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Psychological Review

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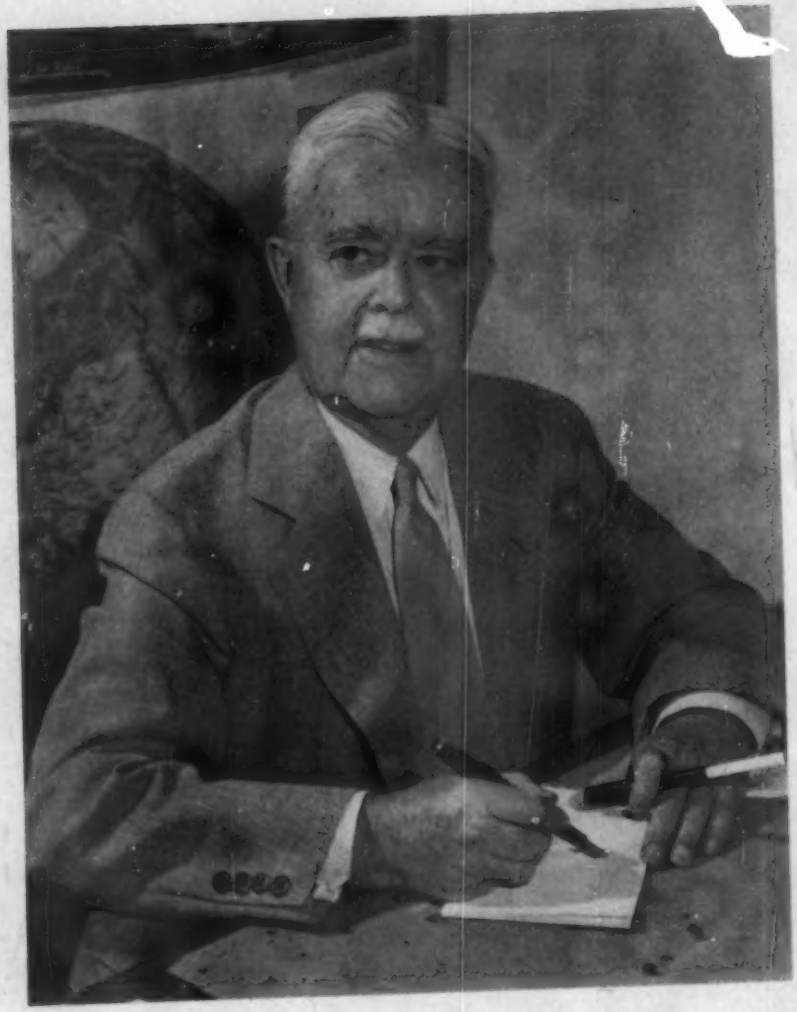

Roswell P. Angier.

UNIV. OF MICH.

VOL. 54, NO. 3 MAY, 1947

THE PSYCHOLOGICAL REVIEW

ROSWELL PARKER ANGIER

The death of Professor Angier on June 25, 1946, brought to a close a life filled with good and enduring accomplishment. As Professor of Psychology, Director of the Psychological Laboratory and Chairman of the Department of Psychology at Yale University, his wisdom, understanding and administrative adroitness influenced students and faculty alike for a full thirty-five years. He died of a heart attack at his home in Tucson, Arizona, at the age of seventy-one.

Roswell Parker Angier was born in St. Paul, Minnesota, on October 21, 1874. By education he belongs to the Harvard tradition, for he was a student there for a period of ten years. He received his A.B. in 1897, his M.A. in 1901 and his Ph.D. in 1903.

At Harvard he fell under the influence of James, Royce, Münsterberg and their contemporaries and he always remembered with respect and a certain sympathetic tenderness the years he spent with them. He was especially fond of James and in later years published two reminiscences of that great American psychologist which gave evidence of keen insight and sympathetic understanding. His 'Another Student's Impressions of James at the Turn of the Century' published in the PSYCHOLOGICAL REVIEW in 1943 gives a series of impressions of James during the years 1893 to 1903 when Angier was at Harvard. The review of 'Perry's Thought and Character of William James,' which appeared in the *Psychological Bulletin* in 1936, served as a vehicle for many of Angier's impressions and interpretations of James's psychological and scientific thought.

These early days at Harvard left a lasting desire in Angier for the historical, the philosophical and the systematic approach to psychology which gave specific direction to his graduate teaching and to his own literary pursuits.

Following the awarding of his doctorate, Angier spent the years from 1903 to 1906 studying at Freiburg and Berlin. During this interval he published a series of experimental studies in the psychophysiology of vision.

His doctoral dissertation dealt with some problems of experimental aesthetics. His discussion of the 'Aesthetics of Unequal Division,' a portion of the dissertation published in the first volume of the *Harvard Psychological Studies*, attacked the problem of the 'golden section' from the purely empirical and experimental point of view. He suggested that eye movements may have the determining effect in the aesthetic experience. It is not strange, therefore, that the papers published in Germany dealt with visual phenomena. One of the first to appear in the *Zeitschrift für Sinnesphysiologie* was the 'Vergleichende Messung der kompensatorischen Rollungen beider Augen.' This was followed by several similar studies including one with Trendelenburg on mixtures of complementary colors, and an important paper published when Angier was Assistant in the Physiological Institute in Berlin entitled 'Über den Einfluss des Helligkeitskontrastes auf

Farbenschwellen.' These papers represent a compact and unified group of experimental studies on basic visual phenomena.

On his return to the United States in 1906, Angier was appointed Instructor in Psychology at Yale University. In 1907 he married Genevieve Severy and they had three sons. The Angier home in New Haven remains in the memories of most of the graduate students in the Psychology Department as a cheerful and kindly haven for all who experienced its hospitality.

In 1908 Angier was advanced to Assistant Professor of Psychology. In 1917 he was awarded the honorary M.A. by Yale and appointed to a full professorship in psychology which he held actively until his retirement in 1941. In the meanwhile he had been appointed Acting Director of the Psychological Laboratory in 1909 and became its Director in 1914.

The years from 1906 until 1917 were primarily devoted to academic pursuits and psychological study. With his return to the United States from Germany in 1906, Angier's interests in the systematic and philosophical aspects of psychology reasserted themselves, and his publications deal with such subjects as 'Attention' and the 'Thought Process.' His work was characteristically more critical and comparative than creative. The continuity of this period was broken by the first World War in which he served as Captain in the Sanitary Corps.

Following the war, Angier entered the 'administrative' phase of his career which covered the years from 1920 to 1941. From 1920 until 1925 he served as Dean of Freshmen. In 1928 he was appointed Chairman of the Department of Psychology and in 1937 became Associate Dean of the Graduate School of Yale University. These posts he held until his retirement. In the meanwhile he served as Chairman of the Institute of Psychology from 1924 until 1928 and as a member of the Executive Committee of the Institute of Human Relations from 1929 until 1932.

During these busy years there was little time for experimental work or for publication in the professional field. Angier's continued interest in the systematic problems of psychological thought found expression primarily in his graduate course in the Historical Origins and Development of Psychology. To this seminar he brought not only his deep appreciation for and familiarity with the historical approach and philosophical implications of various psychological movements and ideas, but as well the new insights which his continued historical research brought to him. The various threads of psychological thought were traced with clarity and profound scholarship. This was especially true of the psychology of the Middle Ages which he dealt with in a superb manner. Angier's students long urged him to publish at least his lectures on the Psychology of the Middle Ages, but the press of administrative detail was too heavy, and after his retirement he devoted himself exclusively to another and highly circumscribed field of psychological thought. To all who attended his seminar, the history of psychology will always be associated with good-fellowship, profound scholarship, sparkling humor, keen insight and the odor of hot tea. It was his ability to quicken, arouse and stimulate his students which characterized Angier as an outstanding teacher.

His greatest gift lay in his ability to deal with students. As Chairman of the Department of Psychology, Dean of Freshmen and later as Associate Dean of the Graduate School this aptitude was recognized and given play. His long academic experience, particularly at Yale where his advice and guidance were sought, together with his personal qualities of sincerity, integrity and sym-

pathy, combined to produce the kindly 'R. P. A.' to whom any student could go in time of stress or anxiety and be sure to receive warm and wise counsel. Under such conditions the purely academic 'research and publish' psychologist disappears, and in his place appears the practical guidance psychologist whose time and effort are consumed in the less tangible but inestimably productive activity of producing scholars and men of psychological wisdom.

It would be impossible to say how many have profited from R. P. A.'s generosity of time and consideration, but as the years go by, one is increasingly impressed with the great influence which he exerted on the students at Yale. There are even those who must owe to him the financial aids which made continued study possible, and it can be said with certainty that some of this aid he provided from his own income with no word to the student concerning the source. He was generous and kindly, but he never sought either recognition or reward for these virtues.

In 1941 Professor Angier retired from the University and moved with his wife to Tucson where they lived quietly in the Arizona sunshine. During the last five years of his life he continued to work on the *History of Motivation* which had long occupied his attention. He was interested in tracing the psychological history of motivation from the Greeks to the beginning of the twentieth century and in following through, separately, the various modes of thought in that field. Unfortunately there was not time for him to complete his work. There remains a great quantity of notes which might form the basis of a history of motivational theories, but only two chapters were written at the time of his death, and without his spark of insight as a guide, there is no way of knowing how he would have interpreted the facts which he had collected or how he would have related the ideas which he had traced. He worked with painstaking care, writing each page over and over until it satisfied his demands for clarity and conciseness. One can only wish now that he had been less concerned with the final form of his work and had given us a broad outline of his interpretative thinking in the field. We would then at least have the essence of his labor. As it is, there remains a mass of data unilluminated by his scholarly thought.

During the war years he was a volunteer worker with the Red Cross in Tucson and was the delight of all who knew him. He continued in retirement and in that community to exhibit the understanding, humor and tireless effort which had characterized his professional life.

When one looks for tangibles on which to base an evaluation of Angier's professional life, one finds a well-worn and annotated copy of James's *Principles*, a group of experimental studies printed in German, a superlative file of notes on the history of psychology, an unfinished manuscript of a history of motivation, and a portrait which hangs at Yale. These are but small tokens of so full a life. Yet behind them stand as intangible and unmeasurable achievements the enviable reputation of the Department and Laboratory of Psychology at Yale University which grew under his guidance, the fame of the Institute of Human Relations which profited by his advice and counsel, and the strength of both the Graduate School and Yale College which are more enduring today by virtue of the life which Angier devoted to them. And the many of us who trod Yale's halls in memory stop in his office for a moment to express our respect and affection for the man who renounced the laboratory to be our friend.

ROBERT T. ROSS
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THE PROBLEM OF PRIMARY STIMULUS GENERALIZATION

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The genuineness of primary stimulus generalization has recently been seriously challenged (12) as not being in agreement with experimental facts. It accordingly becomes necessary in the interest of the advancement of science to reëxamine the available evidence with care, and to secure such additional evidence as is possible.

Preliminary History of the Problem

Pavlov, presumably some time previous to 1910 (15, p. 114), made the empirical discovery that salivary reactions in dogs conditioned to a particular stimulus were evokable by other stimuli but to a diminished degree, the magnitude of the response constituting a falling gradient the height of which at any given point is a decreasing function of the difference between the stimuli in question. In this connection Pavlov states (15, pp. 112–113):

"The successful development of analysis of external agencies by means of conditioned reflexes is always preceded by what we call a 'period of generalization' (which may possibly be regarded as some form of synthesizing activity).

For instance, if a tone of 1000 d.v. is established as a conditioned stimulus, many other tones spontaneously acquire similar properties, such properties diminishing proportionally to the intervals of these tones from the one of 1000 d.v. Similarly, if a tactile stimulation of a definite circumscribed area of skin is made into a conditioned stimulus, tactile stimulation of other skin areas will also elicit some conditioned reaction, the effect diminishing with increasing distance of these areas from the one for which the conditioned reflex was originally established. The same is observed with stimulation of other receptor organs. The spontaneous development of accessory reflexes, or, as we have termed it, *generalization of stimuli*, ..." [Italics ours.]

Subsequently, in discussing the matter of how to secure stimulus discrimination, Pavlov remarked (15, p. 117):

"Formerly we were inclined to think that this effect could be obtained by two different methods: the first method consisted in repeating the definite conditioned stimulus a great number of times always accompanied by reinforcement, and the second method consisted in contrasting the single definite conditioned stimulus, which was always accompanied by reinforcement, with different neighbouring stimuli which were never reinforced. At present, however, we are more inclined to regard this second method as more probably the only efficacious one, since it was observed that no absolute differentiation was ever obtained by the use of the first method, even though the stimulus was repeated with reinforcement over a thousand times. On the other hand, it was found that contrast by even a single unreinforced application of an allied stimulus, or by a number of single unreinforced applications of different members of a series of allied stimuli at infrequent intervals of days or weeks, led to a rapid development of differentiation."

Several years later (1917–18) Anrep, one of Pavlov's pupils and the translator of his major work, made systematic measurements of this 'generalization of stimulation,' as Pavlov calls it, in the field of cutaneous stimuli, determining the generalization gradients for positive learning in terms of the short

trace conditioned salivary secretions of dogs (1). The interval separating the two stimulations involved in each reinforcement was four or five seconds. Anrep found the response gradient to be a decreasing function of the distance of the points stimulated from the point originally conditioned on two animals. His graphs representing two sets of typical measurements of this gradient on a single dog are reproduced as Fig. 1.

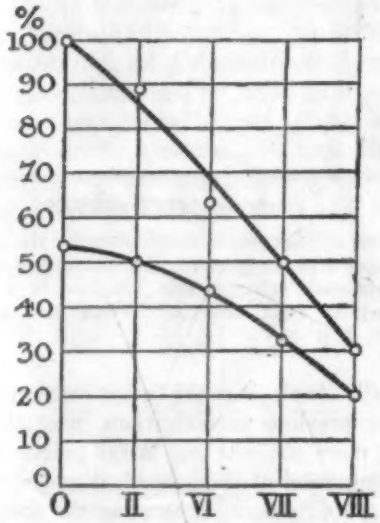

FIG. 1. Two graphic representations of the gradient of cutaneous stimulus generalization of a conditioned salivary reaction in a single dog. The upper curve represents the original gradient, the lower one, the same gradient at a later stage of experimentation after the dog had developed an appreciable amount of 'spontaneous inhibition.' The abscissa values (Roman numerals) represent roughly equal increments in distance from the point on the left thigh originally conditioned, as it extended forward on the right side of the animal to the chest and thence down the front leg. (After Anrep, 1, p. 412.)

Some ten years after the publication of Anrep's study, Loucks (13) published an elaborate criticism of certain experimental work emanating from Pavlov's laboratory. This criticism was directed in the main against an hypothesis of Pavlov's which is that the results of stimulation progress through the brain in a sluggish wave and that this progression is manifested as a parallel temporal phenomenon in the responsiveness of organisms to cutaneous stimulation. The alleged wave-like phenomenon in question we shall, for the sake of clearness in the present exposition, call *irradiation*. This, be it noted, is quite a different notion from the phenomenon represented in Fig. 1. Despite the fact that Anrep seems clearly to refer to the phenomenon represented by Fig. 1 as irradiation (1, p. 407), we shall here call it *generalization*,[1] as Pavlov himself does in the quotation cited above.

Because of the relative freedom of the Anrep data from the usual variability characteristic of mammalian behavior, Loucks refused to accept them as having definite scientific value. In this connection he says (13, p. 43):

"From first hand experience with experiments of this type, from a study of the

[1] Perhaps the clearest difference between the two concepts is that irradiation is conceived as a progressive spread of the effect of stimulation extending over a few minutes only, whereas stimulus generalization is a relatively permanent potentiality of reaction evocation which seems to exist regardless of any recent stimulation whatever. In the terms here specified it may be said that Pavlov believed that stimulus generalization was functionally dependent on, and so, closely related to, irradiation. This supposed causal relationship naturally augmented the tendency to confuse the two concepts. Bass and Hull unintentionally contributed to this confusion by using the term 'irradiation' in the title of their article (2). This was done with a view to making obvious the relevancy of their work to Pavlovian phenomena. They hoped to protect themselves from misunderstanding by inserting a footnote (2, p. 47) categorically denying subscription to any neurological theory whatever, Pavlovian or otherwise, in their use of the terms, 'spread,' 'irradiation,' and 'generalization.' That they were not wholly successful may be seen by consulting Loucks (14, p. 329 ff.).

protocols of other Russian investigators . . . the writer is forced to question the completeness of the data [of Anrep] as standing for the raw measurements, uninfluenced by any selective factor. . . . the writer feels it must be regarded with suspended judgment. . . . If it were not for the fact that this work, taken at its face value, appears of more significance than almost any other in this field, . . . it would not be necessary to raise these questions."

While not particularly sharing Loucks' skepticism as to the scientific significance of Anrep's published results but nevertheless being somewhat disturbed that such a presumably sound and important principle should be called in question even indirectly, Bass and Hull (2) set out to test experimentally the genuineness of the stimulus generalization gradient. They employed in all two groups of eight human subjects, using the conditioned galvanic skin reaction originally evoked by an electric shock. One group of subjects was used for determining the generalization gradient of positive excitation, and one for determining the gradient of extinction effects (inhibition). The Bass-Hull study clearly confirmed the falling gradient of cutaneous stimulus generalization as reported by Anrep. They also reported a closely analogous gradient of the stimulus generalization of extinction effects. Bass and Hull's graph representing their cutaneous stimulus generalization gradient of conditioned response is reproduced as Fig. 2. Despite the use of different types of subjects and very different reactions, the resemblance between Figs. 1 and 2 is striking.

In 1937 Hovland carried out a number of meticulous studies with human subjects on the generalization of positive reaction tendencies to auditory vibration frequency (7, p. 128 ff.), auditory vibration intensity (8), and cutaneous vibration intensity (9). Hov-

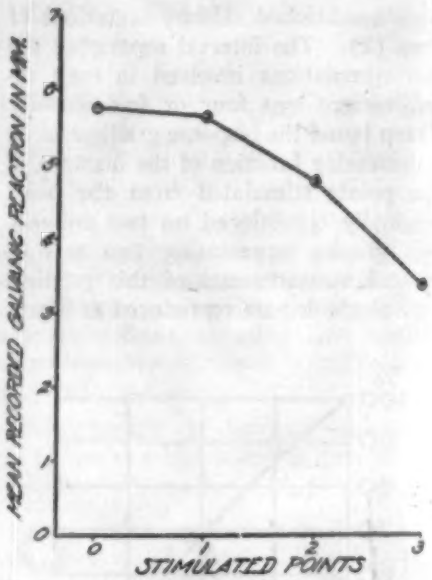

FIG. 2. Composite graph showing the cutaneous stimulus generalization gradient of the conditioned galvanic skin reaction in man based on eight subjects. (From Bass and Hull, 2, p. 60.)

land's work has the great advantage over previous investigations in employing more subjects and in the precise determination of the numerical magnitude of the differences separating the several stimuli involved, which permitted the fitting of equations to his empirical results. His graph of the stimulus generalization of the galvanic skin reaction to excitation by auditory vibration frequency before the use of any additional reinforcement of the conditioned stimulus is reproduced as Fig. 3. It is notable that there is a clear and unambiguous falling gradient having its maximum at the point of reinforcement, i.e., the magnitude of the galvanic response is a decreasing monotonic function of the degree of separation of the stimuli from the stimulus originally conditioned. This, of course, is true of the graphs published by Anrep and by Bass and Hull. There is, however, a distinct difference in the nature

of the curvature of the first two functions (Figs. 1 and 2) and that of Hovland.

Hovland reports experimental evidence (7) that responses evoked by wide stimulus generalization are distinctly more susceptible to experimental extinction than are those evoked by stimuli differing less from the conditioned stimulus, and that the conditioned stimulus is least of all influenced by extinction. The results of a second study in this field, by Hovland, show the generalization gradient on the first, second, and third unreinforced tests of a generalization to tonal intensity. There was found a practically horizontal gradient on the first trial with a fairly conventional gradient of increasing steepness on the second and third trials. A graphic representation is presented as Fig. 4. Hovland also reports a meticulous experiment (7) which yielded a primary stimulus generalization gradient of extinction effects closely resembling that reported by Bass and Hull.

The next experimental study which we notice in the recent history of stimulus generalization was reported in 1942, by Brown (5). In this experiment two groups of albino rats totaling 36 animals were trained to traverse a 60-inch straight runway under a hunger motivation to go to food at the end, where an illuminated ground-glass screen was placed. Half of the animals were trained to go to a screen of .02 apparent foot-candles, and half to a screen of 5000 apparent foot-candles. After 18 reinforced trials of this sort, each group was tested at intervals of 10 minutes with the screen set at .02, at 5.0, and at 5000 foot-candles. The test con-

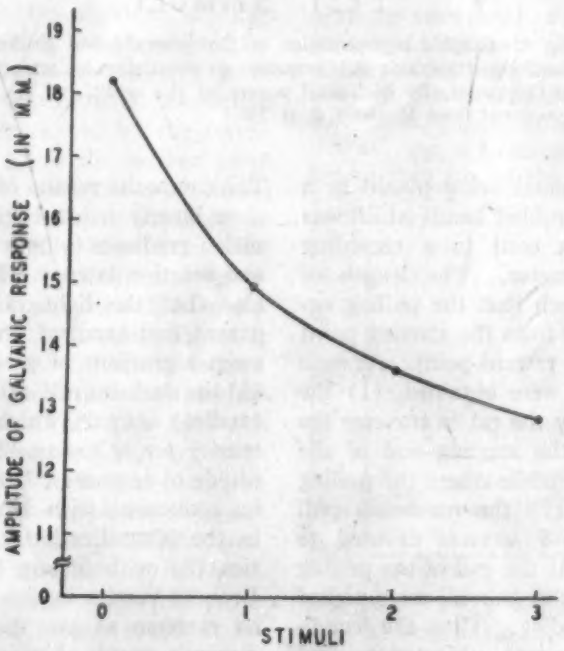

FIG. 3. Composite graph showing the auditory stimulus generalization on the vibration frequency dimension of the conditioned galvanic skin reaction in man. This graph is based on data from 20 subjects. (From Hovland, 7, p. 136.)

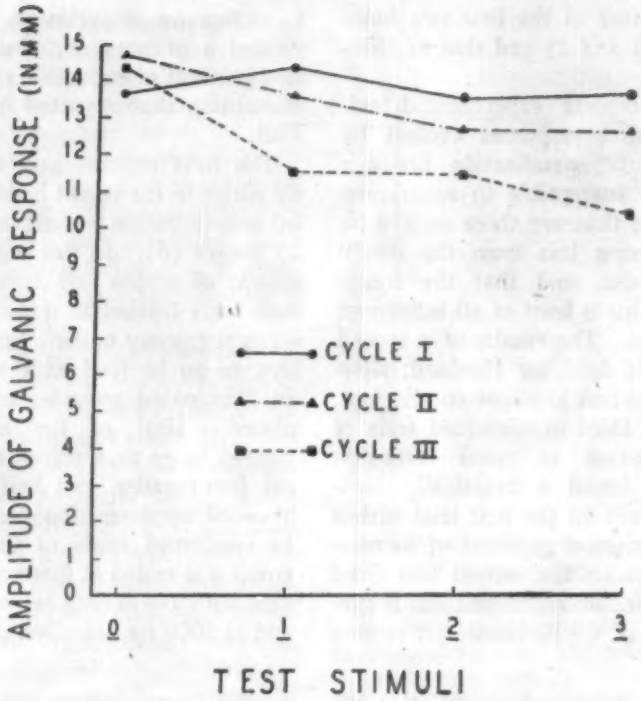

FIG. 4. Graphic representation of the generalization gradients of conditioned galvanic skin responses to tonal stimulus intensity. Note the practically horizontal nature of the results of Test I. (Reproduced from Hovland, 8, p. 286.)

sisted in the animal being placed in a light harness of rubber bands which was connected by a cord to a recording spring dynamometer. The length of the cord was such that the pulling occurred half way from the starting point to the screen or reward point. At each test two scores were obtained, (1) the time required by the rat to traverse the distance from the starting end of the runway to the middle where the pulling occurred, and (2) the maximum pull exerted during 5 seconds devoted to this activity. At the end of the pulling test the animals were in all cases picked up without feeding. Thus the 'conditioned' stimulus was subjected to nonreward exactly as much as were the stimuli which had not been reinforced. Three such tests were given each rat.

The composite results of the 36 animals show clearly marked stimulus generalization gradients in both strength of pull and reaction latency. It may be noted also that the light-trained (5000 apparent foot-candles) animals showed a steeper gradient of generalization than did the dark-trained (.02 apparent foot-candles) animals, which means that intensity *per se* has an effect on the magnitude of response. This observation is in agreement with Hovland's findings in the generalization of sound intensities (8) with human subjects, though Brown's results in this respect are not so extreme as are those of Hovland. Brown's graph showing stimulus generalization as indicated by reaction latency is reproduced as Fig. 5.

A significant detail, which also is in

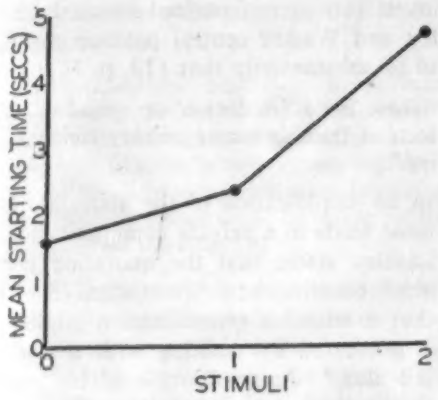

FIG. 5. Composite curve representing the light stimulus generalization gradient as shown by reaction latency by 36 albino rats. Since reaction latency is inversely related to, *i.e.*, is a decreasing monotonic function of, reaction potential, it follows that a generalization gradient plotted as this one is must have its low extreme at the point of reinforcement. (After Brown, 5, p. 216.)

agreement with the Hovland findings noted above, is that the generalized reaction tendencies were markedly more susceptible to experimental extinction than were those evoked by the conditioned stimulus, and the farther away a stimulus was from the conditioned stimulus, the greater the tendency to extinction. Actually the first trial showed a practically horizontal gradient, just as Hovland's did. This means that almost all of the gradient shown in Fig. 5 is due to the differential extinction effects. A graph showing these results is reproduced as Fig. 6, which should be compared with Fig. 4.

The next study to be considered in this survey of the empirical evidence bearing on the problem of stimulus generalization was reported by Wickens. This differs from the other studies included in this summary, because it involves response generalization as well as stimulus generalization. It is included here in the main because its outcome has been supposed, though not by Wickens himself, to be inconsistent with the results of other experiments in this field. Wickens states (21, p. 226):

"The present experiment was designed to test for the possibility of stimulus generalization of the generalized conditioned response. Three groups of subjects were conditioned to tonal stimuli. Shock served as the unconditioned stimulus, and finger extension was the conditioned response. The Ss's hands were then turned over and the tone alone was sounded. For one group the same tone was used, for another the test tone differed by one octave from the training tone, and for the third group a two-octave difference was employed."

The outcome of the experiment in so far as it concerns us here is represented graphically in Fig. 7. In regard to this Wickens states (21, p. 226):

"The data show some tendency for the same tone to be more effective in eliciting the transferred conditioned response than were the generalized tones, if direct comparison of the means is made. Treatment of the data by an analysis of variance, however, produces an F-ratio of less than one. Thus, there is no statistical evidence that any one of these stimuli are more effective than any of the others. This re-

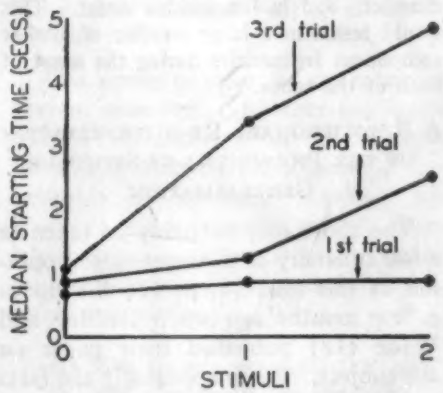

FIG. 6. Graphic representation of the stimulus generalization gradients based on reaction latency as produced by different numbers of unreinforced reaction evocations. Figure 5 is the composite of these three gradients. (Reproduced from Brown, 5, p. 218.)

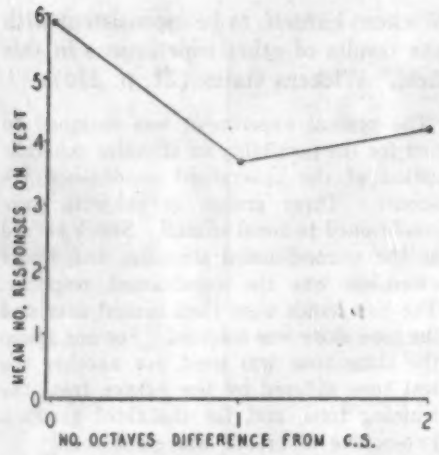

FIG. 7. Graph representing the mean stimulus generalization tendencies of what is in the main a generalized conditioned hand response as a function of the auditory octave. Note that both generalized responses are lower than those to the conditioned response, but that the value at two octaves is higher than at one octave. (Plotted from data published by Wickens, 21, p. 225.)

sult is probably to be expected on several counts. Humphreys has shown that losses in the magnitude of the response during stimulus generalization are not great if the test tone bears an octave relationship to the training tone. Secondly, it will be recalled that the tonal stimulus was brief in duration, and had a sudden onset. This would result in a large number of similar component frequencies during the onset of each of the tones."

A REVOLUTIONARY RE-INTERPRETATION OF THE PHENOMENA OF STIMULUS GENERALIZATION

The above may probably be taken as a fair summary of the consensus of opinion in this field (6, p. 176 ff.) up to a few months ago when Lashley and Wade (12) published their paper on the subject. While apparently the facts as reported by the various investigators are not disputed (with the possible exception of Anrep's results), a radically different interpretation is given to them. Insofar as it is closely related to the investigations summarized above, Lashley and Wade's central position seems to be substantially that (12, p. 74):

"There is no 'irradiation' or spread of effects of training during primary conditioning."

In an amplification of the above statement made in a private communication, Lashley states that the quotation just cited constitutes a "categorical denial that a stimulus generalization gradient is generated by training with a single stimulus." As an example of his position in this matter he states in the same communication that, "in conditioning a white stimulus on a uniform unvarying ground," he "would be confident that no gradient would develop." Thus the central issue emerges as a concrete and specific one of experimental fact.

There are, however, a number of collateral but subordinate issues which tend to bear on the central problem and which therefore need to be considered:

1. Alternative explanations are given to certain experimental results hitherto regarded as evidence indicating the genuineness of primary stimulus generalization:

(A) it is held that neither the salivary nor the galvanic skin reaction yields dependable evidence for the generalization of training effects because (12, p. 84):

"These indicators [salivary and skin secretions] are cumulative, build up gradually, so that increase in reaction time with difficulty of discrimination results in a graduated intensity of response."

(B) It is held that only lower animals make suitable subjects for experiments on primary stimulus generalization. Likewise, the position is taken that adult human subjects are objectionable for such purposes because for such subjects "the stimulus series represented familiar relational sequences, that initial tests directed attention to

the dimensions investigated." Finally, it is held that (12, p. 75),

"To demonstrate irradiation in primary conditioning the subject must be inexperienced with respect to the stimulus dimension used, in order to rule out any tendency to identify a single stimulus as belonging in a familiar graded series or to use habits of relational thinking."

(C) Apparently Anrep's stimulus generalization results with dogs are dismissed from consideration because of the Loucks criticism of the irradiation experiments reported from Pavlov's laboratory.

(D) Lashley and Wade seem to take the position (1) that generalization can occur only on the basis of a stimulus dimension and (2) that a stimulus dimension is only brought into existence by a comparison of at least two stimuli possessing the same dimension. They say, for example (12, p. 74),

"The 'dimensions' of a stimulus series are determined by comparison of two or more stimuli and do not exist for the organism until established by differential training."

This point is clarified somewhat in a private communication which states that when generalization gradients occur "it is the result of comparison which is an essential feature of discrimination and the formation of differential reactions." The context seems to indicate that one necessary condition for comparison to take place is the reception of the stimuli to be compared either simultaneously or in close succession, *i.e.*, the reception of the two stimuli within a few seconds of each other.

2. It is held that experimental results on gradients of stimulus generalization have not been consistent. In this connection the work of Wickens (21) and that of Razran (16) are cited as being inconsistent, apparently, with the results of Bass and Hull and of Hovland.

3. It is held that evidence definitely contradictory to the gradient of primary stimulus generalization has arisen from certain hitherto unpublished experiments from Lashley's laboratory. These experiments involve (1) a process of simultaneously presented visual stimuli (one figured and the other plain black) in which discrimination was produced by differential reinforcement after which (2) the original figured stimulus was paired simultaneously with a new figured stimulus in the same dimension and discrimination learning was finally effected between these stimuli by differential reinforcement. In contrast to what we above called 'Pavlovian differential reinforcement,' we shall call this simultaneous presentation of the stimuli to be discriminated, 'Lashleyan differential reinforcement.'

4. As an incident to Lashley and Wade's discussion of primary stimulus generalization they remark in connection with their own experimental results (12, p. 79):

"We have not attempted to reduce these 'habit strengths' to the equations of the neo-Pavlovian system. The spurious character of its quantitative and mathematical treatment of learning is illustrated by the definition of its units of measurement, the hab and wat. . . ."

This serves to raise the far more important question of whether any quantitative and mathematical treatment of learning or behavior phenomena is either desirable or possible. Lashley and Wade do not specifically commit themselves on these points, but the general context suggests deep pessimism if not actual opposition.

SOME COMMENTS ON LASHLEY AND WADE'S CRITICISM OF PRIMARY STIMULUS GENERALIZATION EXPERIMENTS

We may now comment on the various attempts made by Lashley and Wade to

explain away the experimental facts hitherto regarded as evidence of the reality of a gradient of stimulus generalization. We shall take them up in the order in which they have been presented above, and under the same numbers.

1. (A) Unfortunately, Lashley and Wade do not make clear the logic behind their objection to the use of the salivary and the galvanic skin reaction as an indication of the existence of gradients of stimulus generalization. Here, as frequently throughout their paper, they content themselves with a brief, dogmatic statement. A process of gradual accumulation of a secretion, even if significantly existent in the experiments, could only produce a constant error in such a way as to cause a spurious appearance of a stimulus generalization gradient in case the stimuli were delivered in the experiments in question, *both* (1) within a few moments of each other and (2) in an order progressing along the stimulus dimension from a remote point toward the point of reinforcement. In the Pavlovian experiments the test trials were usually given at relatively long intervals and apparently in a random fashion (15, pp. 55, 57, 63, etc.). Accordingly, failure to conform to the first of the above conditions would rule out the objection to the salivary experiment. In regard to the galvanic skin reaction experiments, elaborate precautions were taken to eliminate any such constant errors by systematically varying the order of stimulation (2, pp. 56, 62; 7, pp. 134, 135). This failure to conform to the second of the above conditions rules out the objection to the galvanic skin reaction experiments. We must accordingly conclude that Lashley and Wade's hypothesis that the generalization gradients based upon the salivary and galvanic skin reactions are mere artifacts or constant errors owing to some hypothetical cumulative characteristics of these reactions, is without foundation.[2]

(B) It must be confessed that we ourselves have in general a considerable amount of sympathy with the preference for lower animal subjects as contrasted with humans, especially with adult humans where behavior is likely to be influenced by stimuli arising from subvocal speech. However, Lashley's objections on this score have only the status of a mere possibility and this possibility is rather remote owing to the relatively non-voluntary character of galvanic skin reaction. To this difficulty must be added the important empirical consideration that at the first trial in some of Hovland's experiments (Fig. 4) and probably in all of them (though the critical evidence is not reported), the gradient at the first test trial was horizontal, *i.e.*, there was no evident slope at all, exactly as with Brown's rats. If the gradient eventually found were due to the indirect effect of previously formed habits, *e.g.*, of speech, it should have appeared in fully developed form at the very first trial because the speech habits were presumably as fully formed at that time as at the time of the subsequent trials. Actually the gradient occurred only as the result of a number of unreinforced separate-presentation reaction evocations. Therefore Lashley and Wade's objection to the acceptance of data from human subjects, plausible as it

[2] Lashley, in a second communication which he transmitted to the writer of the present article after a perusal of the manuscript sent him upon its completion, makes the following statement which is designed to clarify the position taken in the Lashley-Wade article on this point and, specifically, that portion of it quoted above under 1 (A), p. 126. He says, "My argument concerning the relation of salivary secretion and galvanic response to reaction time was not directed against the existence of a sensory gradient but against the use of these quantitative data as evidence for strength of association."

sounds at first hearing, proves on closer examination not to be in harmony with the relevant facts.

(C) Suppose, now, for the sake of getting on with the argument, we should throw out of present consideration all of the experiments involving galvanic skin reactions and human subjects. There would still remain numerous positive experiments employing dogs and rats. But when we consider the dog experiments we encounter Lashley and Wade's reference to Loucks' criticism of results from Pavlov's laboratory. In this connection it must be recalled that Loucks' criticisms are directly concerned only with irradiation, which is quite distinct from stimulus generalization; stimulus generalization as an empirical phenomenon stands on its own feet wholly irrespective of the genuineness of irradiation. There of course remains the possibility of attributing Anrep's quantitative results to slightly attenuated fraud or nature faking. But even though that hypothesis should be adopted there would still remain the definite statement by Pavlov (quoted above, p. 120) which clearly indicates a stimulus generalization gradient. There would also remain the difficult question of explaining how Anrep could have fabricated in advance a measured gradiant which agrees so closely with the gradient found by Bass and Hull, even though the abscissa points of the Anrep gradient are somewhat arbitrarily spaced.[3]

(D) We come now to what appears to be the final criticism of the stimulus generalization experiments. This is that they may in some way be owing to Lashley's type of differential training made on the basis of direct (simultaneous) comparison of the contrasted stimuli. Now, *in not a single one of the experiments cited above were the stimuli involved in the gradients contrasted in Lashley's sense by being presented either simultaneously or within a few seconds of each other.* Therefore, differential reinforcement in Lashley's sense was not involved in any of the experiments used as evidence of this phenomenon. It is true that Pavlov (in Anrep's translation) frequently uses the expressions 'contrast' and 'differential reinforcement' to designate a systematic reinforcement of the conditioned stimulus and systematic non-reinforcement of evocations of the response by other (generalized) stimuli, but *the stimuli were never presented simultaneously* (15, p. 55; 7, p. 135; 2, p. 52 ff.). The results from Pavlov's laboratory, and those from the Bass-Hull study as shown in Fig. 2, are in part owing to the *Pavlovian* type of differential reinforcement, not that of Lashley. Lashley's simultaneous-presentation contrast hypothesis as to the origin of these gradients must accordingly be dismissed as not being in harmony with the relevant facts.

It remains to be added that the gradients of Hovland (Fig. 3) and of Brown (Fig. 5) did not even involve the Pavlovian type of differential reinforcement; no reinforcement whatever was given between the beginning of the tests and their termination.[4]

[3] In the second communication referred to in footnote 2 above, Lashley explains that his objective in citing the Loucks criticism of the Russian experiments on irradiation was not to question the scientific authenticity of Anrep's published data on stimulus generalization. In this connection Lashley makes the following categorical statement: "I do not question that Pavlov and Anrep got a graded series of responses."

[4] In Lashley's second communication, referred to in footnote 2 above, he points out more specifically his meaning of the terms 'comparison' and 'stimuli' in the present connection (12, p. 74). He says, "The statement quoted [under 1 (D) above, p. 127] does not imply simultaneity or close succession of stimuli. Training with one stimulus leaves a trace, which I think of as nervous patterning

2. We now come to the supposedly inconsistent results of Razran and Wickens. The reference given by Lashley and Wade to Razran has been gone over with great care, but the present writer has been quite unable to find anything which would cast doubt on the reality of the falling gradient of stimulus generalization. Hovland's generalization results are cited by Razran, along with the results from the Russian laboratories, apparently with approval. The article is essentially a contribution to a controversy concerned with responses to explicit *patterns* of stimulation, a matter which is not here under consideration.

The Wickens experiment has already been described. We have seen that the stimulus-generalization means in both cases are less than the non-generalized means, which so far is in conformity with other experiments in the field. Nevertheless, when the rather exacting criteria of the analysis of variance are applied to the data the outcome is not such as to permit the conclusion that a typical gradient is proved. But to say that something is not proved is by no means the same thing as to say that it is disproved or that it constitutes any grounds whatever for rejecting the results of some other experiment. As a matter of fact, computations based on Wickens' published results show that the critical ratio based on the standard deviations involved of the difference between points 0 and 1 is 1.70, and that between points 0 and 2 is 1.41. The first gives as a probability that a difference as great as the one observed and in the same direction would occur by chance alone only about 4.5 times in 100 in the first pair, and about 12 times in the second pair. In a private communication Wickens states,

". . . it is worth noting that the percentage of subjects responding in the groups 0, 1, and 2 is 87, 69, and 62 per cent. Plotted, these data give a gradient similar to that obtained by Hovland. When Chi square is calculated, however, none of these differences reach the 10 per cent level. In summary I would like to state that I do not believe the data of my experiment to be at variance with Hovland's type of gradient. In some respects the data are quite harmonious with his findings, although they do not go so far as to offer any *conclusive* statistical support of his gradient."

Thus the general probabilities in this case, so far as they go, are quite in line with the results of the other experiments in the field of primary stimulus generalization.

3. We come now to the experiments from Lashley's own laboratory. Lashley and Wade report that they carried out numerous experiments, all of which were incompatible with the assumption of stimulus generalization. They say (12, p. 78):

"On the contrary, training with one stimulus fails to produce a significantly greater strength of association with that stimulus than with others on the same stimulus dimension. Under conditions which rule out pre-existing habits of relational thinking, no evidence for any graduated spread of effects of training is obtained."

The most impressive mass of data cited by Lashley and Wade are the ten sets of relatively homogeneous rat experiments. Unfortunately the numerical results of all but two of these experiments are not available for statistical analysis. A private communication states that they were accidentally destroyed. But certain significant things are stated about these results.

and which is rearoused in later trials. The 'comparison' may be between this trace and later stimuli (between a memory image and a stimulus, if I must be anthropomorphic to be understood)." It may be noted that a statement of the expanded meaning of 'stimulus' to include the concept of 'trace' was not contained in the Lashley-Wade article.

(A) Lashley and Wade's experiments yield results which are *opposite* to what is held to be the implication of stimulus generalization by a falling gradient or even a horizontal gradient.

(B) All ten experiments agree in this reverse outcome. In this connection Lashley and Wade remark (12, p. 76):

"In every case a differential reaction was established more quickly when the training involved extinction of the initial reaction to a single stimulus than when that reaction was reinforced. The differences are statistically unreliable but are consistent in all 10 experiments."

Now from a statistical point of view it is quite true that the single toss of a coin does not decide anything any more than does a single statistically unreliable experiment. However, in case a coin is tossed ten times in succession with the same result in each case, it is truly remarkable; the probability that this would occur by pure chance is $(1/2)^{10}$, which is but one chance in 1,024. Taking this report at face value, then, it indicates that a generalization gradient of high statistical reliability was really operating in the experiment but that its slope was in the opposite direction from that implied by the stimulus generalization experiments. Thus Lashley and Wade's experiments have a considerably more revolutionary implication than they have claimed. Moreover, these results flatly contradict Lashley and Wade's own conclusion (quoted above) that no graduated spread of the effects of training is obtained. There purports, on the contrary, to be a genuine spread of the effects of the preliminary training, but in the opposite direction.

This naturally raises the question of whether this generalization with the low end of the slope of reaction potential at the point of reinforcement is contained in Lashley's own theory of behavior. If not, then the outcome of his experiment presents an acute explanatory problem to him quite as much as to those who credit the experiments on primary stimulus generalization. So far as the present writer can discover, Lashley and Wade's assumption is that the central figure involved in the first discrimination phase of their experiments, *i.e.*, a concrete representation of the critical stimulus dimension, has received no learning whatever because not previously contrasted with anything in the same dimension. We find, however, that taken at face value their rat experiments indicate that the conditioned stimulus somehow did acquire a *negative* value—some potentiality which interfered with the generation of an approach reaction on the part of the animals. The brief description of the experiments and the fragmentary data available concerning them make it difficult to hazard a guess as to what could have produced an apparent effect which is opposed to the views of both parties to the issue, and points to a type of stimulus generalization never held by anyone, so far as the present writer is aware.

But the situation as a whole is not so confused and hopeless as the Lashley-Wade experiment might suggest. It happens that there have recently become available some experimental results involving simultaneous discrimination in rats which apply directly to the present issue. Spence (18) reports that he trained 20 albino rats for four days to take a single black (or white) runway to food on a special apparatus (17). After a total of 40 trials of this training the animals were divided into four groups and trained by differential reinforcement to choose either the shade of alley which they were previously habituated to choose, or the opposite shade. The mean outcome of the experiment in terms of number of errors is as follows:

1. Trained on White	1. Trained on Black	1. Trained on White	1. Trained on Black
2. Trained on W + vs. B − (++)	2. Trained on B + vs. W − (++)	2. Trained on B + vs. W − (+−)	2. Trained on W + vs. B − (+−)
.4	7.2	31.4	12.6

It may be noticed, as we have had occasion to point out in certain other experiments on black and white discrimination, that the animals show a special tendency to learn to go to white, the physically more intense of the two opposed stimuli employed. Thus the combination of the first and fourth values given above, where white was the positive stimulus, gives a mean of 6.5 errors, whereas the combination of the second and third values, where black was the positive stimulus, yields a mean of 19.3 errors. However, since this factor is presumably equalized in the two ++ groups as well as in the two +− groups, the two of each are averaged to secure the results of training transfer due to primary stimulus generalization with the effects of stimulus intensity equalized. In the notation employed by Lashley these means are:

$$++ = 3.8 \text{ errors}$$
$$+- = 22.0 \text{ errors}$$

Thus the animals which in the discrimination experiment were trained to choose the brightness to which they had been trained in the preliminary non-discriminatory learning, learned the discrimination with considerably fewer errors than did the animals which had to reverse the habit formed during the preliminary single shade training, the difference being 18.2 errors. A test of this difference by the 't' statistic gives a value of 4.09, which is beyond the .001 level of significance.

4. It is difficult to believe that any behavior scientist would actively oppose the placing of the behavior sciences on a secure quantitative, mathematical basis. On the other hand, it is axiomatic that until reaction potentiality and habit strength as such are measured in appropriate units a true basis for a mathematical systematization does not exist. In this connection reaction potential as such is to be contrasted with various phenomena taken as indicating its presence, *i.e.*, as having a monotonic functional relation to it. A number of such indicators are: (1) the number of reinforcements, (2) the amount of conditioned secretion, (3) the degrees of arc moved by a galvanometer, (4) the number of non-reinforced responses evoked, (5) the number of grams lifted in a conditioned muscular contraction, (6) the number of centimeters moved in a conditioned muscular contraction, (7) the number of seconds involved in the reaction latency, (8) the number of errors occurring per trial in a learning situation, and (9) the per cent probability of response occurrence following a given kind of stimulation. Under favorable conditions all nine of the above measurable phenomena are recognized as indicators of reaction potential or habit strength, but we have as yet no equations for converting any of them into reaction potential or *vice versa*. The situation is analogous to that in the history of heat measurement previous to Joule's classical experiments which demonstrated the mechanical equivalent of heat. But the history of thermometry and calorimetry should warn us that the adequate quantification of the behavior sciences will be neither swift nor easy (3; 4).

In 1943 a largely programmatic sketch of what the behavior sciences might be like if so quantified and mathematically systematized was published (11, p. 327). Since that time,

work toward realizing this program has gone forward continuously in the writer's laboratory. It may merely be said here that, utilizing substantially the logic implicit in the classical work in psychophysics as elaborated by Thurstone (20), we appear to have made numerous approximate quantitative determinations of reaction potential ($_sE_R$) as such. The unit of measurement used so far (as might be anticipated from the relationship of psychophysical determinations and scaling) is the standard deviation of the probability function or, what in the present system is the same thing, the σ of the oscillation function ($_sO_R$) (11, p. 304 ff.). Several papers devoted to the empirical aspects of this subject are at the present time in preparation, one by Sprow (19) being in press. It may be added that a tentative equation has even now been derived for converting the reaction latencies, into reaction potential ($_sE_R$). Space is here lacking for a detailed explanation of the methodology employed to do this, except to say that it is analogous to the somewhat abbreviated one used by Sprow.[5] The equation in question is

$$_sE_R = 2.85\ {}_st_R^{-.483} - .60.$$

[5] In connection with the discussion of the mathematical treatment of learning, Lashley in the second communication mentioned in our footnote 2 amplifies the statement of Lashley and Wade quoted above (p. 131) as follows: "I certainly do not oppose the placing of behavior sciences on a secure quantitative mathematical basis. But the making of consistent mathematical constructs and determination of their fit to observable phenomena are different matters. *The implication of my . . . footnote is that the values or constants necessary for your equations are indeterminable.*" (Italics added.) We have in the italicized sentence quoted just above a confident and unequivocal, though characteristically unsupported, statement that the scientific goal in question is a human impossibility. Lashley thus deliberately puts himself on record. The next few years should begin to show whether his intuition on this point is correct.

SUMMARY AND CONCLUSIONS

We may now sum up the conclusions at which we arrived in the preceding pages.

1. Early in the conditioning process primary generalization is complete, or nearly so, within the stimulus field originally conditioned, *i.e.*, the gradient of generalization shows little or no slope downward from the point conditioned (Pavlov, Hovland, Brown).

2. With continued reinforcement of the conditioned stimulus, a downward sloping gradient of primary stimulus generalization of reaction strength gradually and spontaneously develops (Pavlov).

3. This generalization gradient of reaction strength is a monotonic decreasing function of the magnitude of the differences between the conditioned stimulus and the unconditioned stimuli (Pavlov, Anrep, Bass and Hull, Hovland, Brown, Wickens).

4. Primary stimulus generalized conditioned reaction tendencies are much more susceptible to experimental extinction than are reactions actually conditioned to the response; and the wider the generalization, the greater the susceptibility. Equal numbers of unreinforced reaction evocations by all the stimuli involved (including the originally reinforced stimulus) accordingly result in a marked steepening of the generalization gradient (Hovland, Brown).

5. Another very effective method of steepening the primary stimulus generalization gradient in the case of the single-presentation or Pavlovian type of discrimination is differential reinforcement or contrast, *i.e.*, systematic reinforcement of the conditioned stimulus while systematically extinguishing one or more generalization stimuli (Pavlov, Hovland).

6. Stimulus intensity as such has a marked tendency to evoke reactions in the stimulus generalization situation

(Hovland, Brown). This principle deserves a more explicit place in a comprehensive theoretical formalization than it has yet received.

7. The stimulus generalization gradient manifests itself with rats even in the complex differential reinforcement with simultaneously presented stimuli as employed by Lashley and Wade, and apparently directly contrary to their reported findings (Spence).

8. Differential reinforcement with simultaneous stimulus contrast critically involving the dimension in question is not necessary as a pre-condition of primary stimulus generalization (Pavlov, Anrep, Bass and Hull, Hovland, Brown, Wickens).

9. Lashley and Wade's statement of their belief in the absurdity of attempting to reduce the habit strength and reaction potential resulting from learning to mathematical treatment in the form of quantitative equations, ironically comes at the moment (June, 1946) when this seems actually to be taking place.

In conclusion the author wishes to thank Dr. Lashley for an advance copy of the Lashley-Wade manuscript, and for his kind invitation to write a reply to it. This courtesy greatly facilitated the relatively early preparation of the present study.

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THE BEHAVIOR OF ATTITUDES

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There is no question that the subject of attitude and attitude measurements is important in sociology and social psychology. Social scientists continue to discuss the nature of attitudes in articles like this, to conduct experiments which show that behavior is affected by attitude, and to measure attitudes for theoretical or practical purposes. The problem of what an attitude is and how it functions, nevertheless, persists and—as many writers on attitudes likewise point out in their introductory paragraph—little explicit agreement is apparent in the published literature.

The purpose of this paper is not to criticize other definitions or usages of the term but systematically, if partially, to relate the concept of attitude to what is known as behavior theory (17).[1] Almost all writers, no matter what their bias, agree that attitudes are learned. If this is so, then the learning, retention, and decline of an attitude are no different from the learning of a skill, a piece of prose, or a set of nonsense syllables; and they must also involve the problems of perception and motivation.

Immediately it is necessary to raise and answer the question as to why a simple, commonsense, ubiquitous concept like attitude should be translated into semi-technical jargon. There are at least two answers to the question. The first and less important answer involves scientific methodology: it is thought desirable to bring as many terms as possible relating to a field of research (in this instance, human behavior) within one universe of discourse. Misunderstandings result from using one set of concepts to describe perception, learning, and motivation on one level and another set on a different level. Even if some sociologists who use the concept 'attitude' are not attracted by the terminology of behavior theory and if they remain inclined merely to assume perception, learning, and motivation without inquiring into the details of the processes, unified knowledge concerning human behavior requires that there be a connection between what the sociologist studies or measures and what the psychologist studies or measures. Secondly and of crucial importance, a tentative translation of a term from one level to another and into an already and perhaps better developed theoretical system is fully justified if thereby inadequacies on the higher level can be pointed out. Murray and Morgan (27), for example, have recently shed new light on the nature of attitude by incorporating the closely related concept of sentiment into the former's previous conceptualization of personality.

The procedure to be followed in this paper is as follows:

1. A definition of attitude in behavioral terms will be given. It is felt that such a definition represents an advance beyond the stage of defining an attitude as the subjective counterpart of something in the environment, as a predisposition within the organism, or as being what the attitude scale measures.

[1] The writer is deeply grateful especially to Neal E. Miller as well as to Irvin L. Child, John Dollard, and Mark A. May for their constructive criticisms. He has promiscuously and deliberately borrowed some of their ideas and, as an insignificant token of his gratitude, herewith absolves them of any responsibility for the final product.

The psychological implications of the definition will be made clear.

2. The consequences of this definition and the theoretical structure it assumes will then be summarized briefly by calling attention to the factors which should be known to make a completely adequate analysis of attitudes.

3. Illustrative research employing the concept of attitude will be critically examined in terms of these factors. Such a detailed examination seems preferable to surveying the attitude literature in general terms.

DEFINITION OF ATTITUDE

Attitude is defined as *an implicit, drive-producing response considered socially significant in the individual's society.* This definition states, in effect, that from the psychological point of view attitude is an implicit response with drive strength which occurs within the individual as a reaction to stimulus patterns and which affects subsequent overt responses. Since psychologists and other social scientists sometimes disagree concerning the nature and attributes of an implicit response of this type, the definition is therefore elaborated and broken down typographically into the phrases and clauses requiring further definition, elaboration, and discussion:

"An attitude is:

(1) "an implicit response

(2) "which is both (a) anticipatory and (b) mediating in reference to patterns of overt responses,

(3) "which is evoked (a) by a variety of stimulus patterns (b) as a result of previous learning or of gradients of generalization and discrimination,

(4) "which is itself cue- and drive-producing,

(5) "and which is considered socially significant in the individual's society."

1. *"An attitude is an implicit response. . . ."* By an implicit response is meant a response occurring within the individual and not immediately observable to an outsider. Motor 'attitudes' like the physical set of the runner before the starter's gun is fired, therefore, are not included within the universe of discourse being analyzed because they are not entirely implicit and hence are instrumental rather than mediating acts. The semantic connection of such sets with the concept of attitude employed by social scientists is considered to be largely fortuitous from an historical standpoint.

Overt behavior that is observable to an outsider may be affected by the evoked attitude but is not here defined as the attitude itself. Attitude refers to the individual's immediate but implicit response to a stimulus pattern and his consequent tendency to respond still further as a result of that implicit response. Such an implicit response may be conscious or unconscious, distinctly verbal or vaguely proprioceptive. What is expressed results not from the attitude alone but represents, as will be indicated, another response in a behavior sequence—an overt one—which is a function of the attitude-response and other tendencies within the individual.

2(a). *"An attitude is an implicit response which is . . . anticipatory . . . in reference to patterns of overt responses. . . ."* An anticipatory response—called also an antedating response by Hull (17, p. 74)—is one which originally preceded another rewarded response and which, as a result of being associated with or producing this reward, has been reinforced so that it occurs before its "original time in the response series" (25, p. 49). If an individual, for example, dislikes a fruit or a person, he tends to avoid eating the fruit or meeting the person. Originally the avoidance occurred only after actual contact had been established and after

that contact had proven to be punishing and the withdrawal to be rewarding. When a thorough investigation reveals no actual prior contact, some process of generalization or discrimination must have occurred since all behavior has antecedents. The possibility of exceptions under different psychological conditions must be noted, even in these trivial illustrations: the fruit may be eaten anyhow if the individual is very hungry and the person may be met and greeted if social circumstances so require.

The conditions under which a response moves up in the behavior sequence and becomes anticipatory have so far been determined concretely in relatively simple situations. It has been shown (25, p. 79), for example, that the closer the response is in the series leading to the reward, the more likely it is to be learned and then subsequently to antedate other responses not leading to the reward. This principle suggests why few objects or individuals in society fail to arouse attitudes. Originally the individual has had to react to them or has been taught to react to them in the course of being socialized. One of the responses leading to the goal response (which by definition involves reward or the avoidance of punishment) has implicit components, is reinforced, and is here called an attitude.

2(b). *"An attitude is an implicit response which is . . . mediating in reference to patterns of overt responses. . . ."* Whereas the anticipatory character of attitude indicates its temporal relation to a goal, its mediating attribute calls attention to its functional connection with that goal. A mediating response is made in an attempt to increase the likelihood of the occurrence of reward rather than punishment in connection with a goal response. In reasoning, for example, implicit responses intervene between the original stimulus pattern and the goal response and may assist the individual in achieving that goal. Attitudes can be evoked so easily because as mediating responses involving only language, imagery, or proprioceptive reactions they need not conflict with the overt behavior of the individual or with his environment.

The mediating function of attitude has led May to suggest that attitude is "a kind of substitute goal response" which "arises when the goal response cannot be immediately and easily made" (24). This attribute, which has also been suggested by other writers (*e.g.*, 3, pp. 425–6 and 28, pp. 28–9), does seem to characterize certain attitudes. The individual who dislikes another person is restrained or restrains himself from hurting his antagonist; instead he makes an implicit response involving aggression and feelings of avoidance or repulsion. It is felt, however, that all attitudes cannot be so characterized. The liked object, for example, evokes an implicit response which facilitates rather than acts as a substitute for overt behavior in reference to it. Overt behavior, in short, may be mediated by attitudes almost immediately and there need not necessarily be a conflict or a restraint before the attitude is evoked.

Three consequences arise immediately from this conceptualization of attitude. In the first place, it appears psychologically futile—as Sherif and Cantril have indicated (33, pp. 304–5)—to attempt to classify attitudes. Responses can be characterized in so many different ways that a simple dichotomy or trichotomy usually must be willfully stretched if it is to include all types of behavior. The response defined as attitude might be called positive or negative or be said to involve approach or withdrawal were it not for the fact that these terms then require further definition which cannot be consistently or usefully applied to all situations. Both

approach and avoidance, for example, may be but are not necessarily involved in what has been called a neutral attitude or in any attitude for that matter. It seems better, in short, to apply *socially* useful labels to attitude as the need arises, in order to indicate the direction in which the individual thereby is oriented; but it should be clearly recognized that the labels have social and not psychological significance. A psychologically important distinction, however, is that between general and specific attitudes (5), a distinction which refers to the stimulus patterns evoking the response or to the evoked responses.

In the second place, reference is made in the definition to "patterns of overt responses." For reasons hereafter suggested, overt behavior can seldom be predicted from knowledge of attitude alone. Under varying conditions within the individual, a given attitude can mediate a repertoire of overt responses. A favorable attitude toward a social institution, for example, can mediate innumerable responses connected with what is considered to be the welfare of that institution.

Then, thirdly, this definition of attitude emphasizes its acquired or learned character. There are no psychic rays which enable the investigator, even though he be equipped with a poll or a scale, to determine the 'strength' of an attitude, the overt responses with which it has become associated, or its present functioning within the personality. Such knowledge can be obtained only from knowing approximately under what conditions the attitude was acquired in the first place and the extent to which it secures present and future reinforcement. The learning process, therefore, is crucial to an understanding of the behavior of attitudes.

The nature of that process cannot be ignored in a treatise on attitude as it has been by Sherif and Cantril: "Just what the psychological or physiological mechanisms of this learning may be are irrelevant to the present discussion," they write in their articles on "The Psychology of 'Attitudes'" (33, p. 302). It is difficult to see how psychologists can call these mechanisms 'irrelevant' and still contend, as the authors do in the introduction to the same articles, that the 'task' of the psychologist is to "give an adequate account of the psychological mechanisms involved in the formation of an attitude in any individual" (33, p. 295). The authors rest their case by stating that they "do not need to take sides in favor of any learning theory" (33, p. 307). Of course, as they write, "the primary stage in the formation of attitudes is a perceptual stage," which is merely saying that there must be a stimulus and that the stimulus must be perceived before there can be any kind of learning.

In contrast, Allport (1, pp. 810–2) has set the problem of the genesis of attitudes in terms which explicitly suggest the need for learning theory. His often quoted summary of the literature indicates that attitudes may be formed through "the integration of numerous specific responses of a similar type"; "individuation, differentiation, or segregation" of experiences; 'trauma'; and the adoption of 'ready-made' attitudes of others. The genesis of almost any attitude is undoubtedly more or less unique. Society sets the rewards and punishments regarding much of overt behavior; the individual being socialized then is forced into the learning situation; although he reacts to the situation uniquely, one of the end products—the attitude—he may share in large degree with others.

3(a). *"An attitude is an implicit response . . . . which is evoked by a variety of stimulus patterns. . . ."* The stimuli evoking an attitude may be in

the external world or within the individual. The latter range from a verbal response to an autonomic disturbance or a drive. The existing literature on attitudes testifies to the fact that the stimuli may be various. Such is the assumption behind any attitude scale in which a variety of situations is judged or behind a distinction like that between specific and general attitudes.

The arousal of an attitude involves two traditional problems in psychology, those of perception and learning. The two are interrelated and can be separated only for purposes of analysis. Perception indicates that the individual is responding because he has previously paid attention to or been oriented toward certain stimuli which then affect his sense organs and thus evoke his attitude. Learning in this connection emphasizes the reasons in the past history of the individual which have brought about the bond between the stimulus pattern and the attitude.

Gestalt psychologists especially point out that paying attention and then perceiving occur in many instances as a result of the individual's set to respond: letter boxes are not noticed unless the person has a letter to post, etc. Writers on attitude are fond of recalling that the tradition of set is somewhat hoary in academic psychology, in order to demonstrate their own respectability and their acquaintance with long German terms like *Bewusstseinslage*. In other words, perceiving depends upon drive or set (which orients the individual to respond to certain stimuli and then to respond to them in a particular way) as well as upon the arrangements of the external stimuli. Attitude may be included among the sets determining both the orientation of the individual as well as the kind of perceptual response he makes: The southerner notices incipient aggression in a Negro which a northerner will overlook.

It must be recognized, however, that in some situations the individual's attitude is not evoked until the stimulus has been actually perceived: It affects perception after its arousal but does not orient him originally in the direction of the stimuli involved. The southerner who has learned to discriminate incipient hostility from genuine docility among Negroes can make the discrimination when confronted with a Negro. It is on the basis of this discrimination that his attitude toward the Negro is or is not aroused. If he goes about the streets looking for hostility in Negroes, he may be set to make the discrimination not necessarily by his attitude toward Negroes or by his ability to detect such behavior but by some particular drive which has been previously aroused and which may or may not involve Negroes. To say that he searches for aggression solely because of his attitude toward Negroes is to fail to distinguish him from another southerner who has a more or less identical attitude (so far as content and even 'affect' are concerned) but who has no 'chip on his shoulder.' In this case the sensible problem remains of accounting for the 'chip.'

In controlled experimental situations dedicated to observing the behavior of attitudes, the attention of the subjects is secured by the experimenter not through arousing the attitude being studied but through the evocation of some other drive. Subjects perceived the autokinetic phenomenon of Sherif (30) or they looked at Seeleman's photographs of white and colored individuals (29) because they had agreed to cooperate with the experimenter. This was the drive which oriented them in the experimental situation. Then their attitudes were aroused and these attitudes affected both their perceptual responses and the reports they gave the experimenters. Later on in the experi-

ments, their evoked attitudes may have had sufficient drive strength also to orient them selectively.

3(b). *"An attitude is an implicit response . . . which is evoked . . . as a result of previous learning or of gradients of generalization and discrimination. . . ."* The previous section has indicated the possible relation between perception and the evocation of an attitude and therein it was suggested that an attitude can almost always be aroused by a variety of stimuli. Here it is stated that previous learning determines whether or not particular stimulus patterns will evoke the attitude. Some stimuli come to arouse an attitude after a relatively simple process of conditioning has occurred. As a result of being originally present in the situation, for example, the wrapper of the disliked fruit or the signature of the disliked person may produce an anticipatory response which mediates the goal response of avoiding it or him. Other stimuli evoke or fail to evoke an attitude not because of their presence or absence in previous situations but because they fall along a stimulus gradient of generalization or discrimination. If an individual likes or dislikes one particular Negro, there is stimulus discrimination; if all Negroes are involved, there is stimulus generalization along a gradient, for example, of skin color; if only certain 'types' of Negroes arouse the attitude, there is generalization and discrimination.

Frequently attitudes are thought to be puzzling and mysterious because they can be aroused when the psychologist, the sociologist, the layman, and the individual least expect them to be. The stimulus pattern, for example, may be a word or a sentence, or some other symbol (like a flag, a face, or a gesture) which represents only a portion of the original stimulus pattern. The behavior of attitudes under these circumstances, however, becomes more intelligible when the implications of the "gradient of generalization and discrimination" are understood. This gradient is especially efficacious in the case of attitude since language can readily perform the function of mediating generalization and discrimination. Cofer and Foley (11), for example, are able *a priori* to list over fifty gradients along which generalization or discrimination from a single word can conceivably occur in purely formal (*i.e.*, in non-idiosyncratic) manner. The word 'Negro,' for example, through previously learning may become the one part of the original stimulus pattern which evokes an attitude regarding Negroes. Without any difficulty there can be a semantic gradient including words like 'colored,' 'African,' and the various epithets and appellations applied to this group. Through a slightly more complicated process, eventually 'zoot suit' may arouse the same attitude: if the individual has associated this type of clothing with Negroes, the response of seeing such clothing or hearing its name evokes the internal response of 'Negro' which in turn has the property of arousing the attitude. This phenomenon has been called secondary stimulus generalization or acquired cue value. Similarly, thinking can aid generalization and discrimination.

Cantril in his work on attitudes fails to comprehend this stimulus gradient involved in most if not all learning and conditioning. He states, for example, that what he calls "a standard of judgment" must not be "confused with a conditioned response" (8, p. 25). What he means by "a standard of judgment" or a "frame of reference" he never makes operationally clear, although they seem to be related to attitude since they are employed profusely in his two articles on the subject. It may be that terms like these—to use Cantril's own words in reference to 'conditioning'—

are "so loosely employed" that they "explain nothing at all" (8, p. 55). His conception of a conditioned response is indeed narrow: "a specific reaction to a specific stimulus" (8, p. 25). In spite of a general, noncommittal reference in a footnote to a standard book on conditioning (that of Hilgard and Marquis) and an irrelevant quotation from A. N. Whitehead, this simplification of behavior theory leads him to conclude that such a theory is "by no means adequate to explain the apparent meaningfulness of man's experience" (8, p. 56). More specifically, for example, he maintains that the "analysis of the backgrounds of the people who became panicky listening to the Hallowe'en broadcast [of Orson Welles in 1938] clearly shows that in no way had they been specifically conditioned against Martian invaders' mowing down people on this planet" (8, p. 25). The point of Cantril's study (7), however, is to discover the previously existing attitudes and traits which made people prone to panic or sanity. After discovering many of them with admirable ingenuity, he gives a commonsense 'explanation' of the behavior; *i.e.*, he uses his particular vocabulary to describe what happened (7, pp. 190–201). His failure to apply Chapter 8 of Hilgard and Marquis (15) to his own data prevents him from realizing that at the time of the broadcast people had learned to respond with anxiety to various stimuli as a result, for example, of the 'war scare' associated with the Munich crisis (*cf.* 7, pp. 159–60). Certain Americans, some of whose socio-economic and psychological characteristics Cantril has indicated, apparently generalized from these stimuli to a broadcast which of course they had not previously experienced but which nevertheless was sufficiently similar to other stimulus patterns previously evoking the anxiety. Such individuals 'jumped the gun' by behaving—after their attitudes had been aroused by Mr. Welles and his associates—as they would have in the face of genuine catastrophe.

From experiments in other fields of behavior, Hull states that the amplitude of a response "diminishes steadily with the increase in the extent of deviation . . . of the evocation stimulus . . . from the stimulus originally conditioned . . ." (17, p. 185) and that "The strength of the connections at other points of the zone can be determined only from a knowledge of the strength of the receptor-effector connection . . . at the point of reinforcement and the extent of the difference . . . between the position of the conditioned stimulus . . . and that of the evocation stimulus . . . on the stimulus continuum connecting them" (17, p. 187). Whether or not the principle holds true of attitudes has not been tested, so far as this writer knows. To test it, it would be necessary to know "the stimulus originally conditioned," the gradient along which the various 'evocation stimuli' are located, and whether or not in fact these latter stimuli have never been previously reinforced or extinguished. This could be done only by means of a careful life history of various individuals and by some objective measurement of the strength of the attitude.

At any rate, this generalization of Hull has an important bearing on the strength of attitude. The 'strength' of an attitude is almost as ambiguous as the concept of attitude itself. One type of strength that seems important for predicting future behavior of the individual is the *afferent-habit strength* of the attitude, *i.e.*, the strength of the bond between the stimulus pattern and the response which is here defined as attitude. In these terms the afferent-habit strength of an attitude is a function of the number of previous reinforcements as well as the position along the

gradient occupied by a particular stimulus pattern.

4. *"An attitude is an implicit response . . . which is itself cue- and drive-producing. . . ."* Like all implicit responses, attitudes can be said to have stimulus-value, *i.e.*, they arouse other responses. The responses they evoke may indeed be various. They may produce a perceiving response, as has been suggested, and in this sense determine to what other stimuli the individual subsequently responds. They may produce linguistic responses, thoughts, images, or stereotypes. They eventually have an effect upon overt behavior. Here again reference must be made to the habit strength of the attitude, only this time it is a bond between attitude as a stimulus and a response pattern (implicit or overt), rather than between a stimulus pattern and attitude as a response. This phenomenon will be called the *efferent-habit strength* and, like the afferent-habit strength, it too can be strengthened or weakened through learning. The stimulus-value of attitude suggests why investigators have discovered so many different phenomena—aside from overt behavior—associated with attitude.

The definition of attitude refers to the distinction made by Miller and Dollard (25, pp. 22–3) between the cue- and drive-producing characteristics of a stimulus. The numbers used in counting, they point out, have cue value: the number 'six' is a response to the cue of 'five' and in turn acts as a cue to elicit 'seven.' An attitude has cue-value in the sense that it acts as a stimulus to produce another response, but it also is a drive in the sense that its tension is reduced through subsequent behavior leading to a reward. It is proper, consequently, to refer to the *drive strength* of an attitude as well as to the strength of the bond between the attitude and the responses it evokes. An individual, for example, who has not been thinking of food is shown a picture of a type of food he likes. The picture may be said to evoke his favorable attitude toward the food which is a drive whose goal response is eating. If the secondarily evoked response pattern is only salivation and the thought or statement of how he likes the food, the drive may be considered relatively weak. But if he drops what he is doing and goes out to eat immediately after seeing the picture, the drive is relatively strong.

The strength of an attitude, therefore, may refer to its afferent-habit strength, its efferent-habit strength, or its drive strength. Knowledge of an attitude's direction or content is not equivalent to measuring its strength. The attitude possessed by two people, for example, may be more or less identical in formal content as measured by an attitude scale, but its role in determining overt behavior may be quite diverse. In one individual it may be evoked by a restricted stimulus pattern (specific attitude), it may not be frequently evoked (a weak afferent-habit), or the entire pattern of additional implicit and overt responses may be restricted (a weak drive). Any one or more of these conditions may be different in the second individual. Thus the role of the patriotic attitude in the average citizen is different from the way it functions within a professional patriot. Or within the same individual the patriotic attitude may be more easily and variously aroused and lead more certainly to a greater number of responses during a war than when there is peace.

The fate of an attitude over time—whether it persists or changes—is obviously a function of complicated processes within the individual. At least three factors are involved. The first is the reward or punishment associated with the goal response. An attitude will persist when it is constantly reinforced

or it will change when it is partially or wholly extinguished. Its afferent- and efferent-habit strength is then increased or decreased. The efferent-habit strength is a direct function of the frequency with which the attitude has mediated rewarding or non-rewarding behavior as well as of the immediacy and amount of the reward or non-reward. Afferent-habit strength depends ultimately upon efferent-habit strength since reinforcement or non-reinforcement results not from the arousal of the attitude but from the behavior mediated by the attitude; it also depends on the frequency with which particular stimulus patterns have aroused the mediating attitude and the distinctiveness of those patterns. In Hull's words (16), attitudes have the property of 'pure stimulus acts' within a single individual: they are responses which are reinforced "because they, as stimuli, evoke *other* acts . . . which will bring about a reinforcing state of affairs. . . ."

Secondly, there is the factor of conflict with competing drives which may determine the fate of an attitude. The afferent- and efferent-habit strength of an attitude may be great, but its drive strength may be weak in comparison with other attitudes or drives aroused by the same or different stimulus patterns. The individual, for example, may not express his attitude in overt behavior because its expression would be contrary to his general philosophy; but his attitude persists. Finally, there is forgetting which may involve other psychological processes besides extinction through non-reinforcement.

It is possible, consequently, to have an attitude play a less significant role in a personality by diminishing the number of stimuli which evoke it, by having the attitude aroused less frequently, by weakening its drive strength, by setting up stronger competing drives, or by punishing the overt behavior which it mediates. Since the afferent-habit strength is affected by the final goal response, moreover, it follows that the attitude's strength in this respect can remain more or less constant even though its efferent-habit strength vis-à-vis a particular response pattern is weakened: it is only necessary that some reinforcement be obtained by having the attitude mediate other goal responses. The prejudiced individual, for example, can retain his prejudice not by always behaving identically in response to a given stimulus pattern in his society but by behaving differently and with more or less equal satisfaction.

There is a voluminous literature on the subject of attitude change, a good part of which is called experimental (*cf.* 26, pp. 946–80). In general, the approach has been to subject a group of individuals to a collection of stimuli (like a course in race relations, a motion picture, propaganda analysis, or a visit to an ethnic group in a large American city). Their changes in attitude (if any) are ascertained by comparing their scores on an attitude scale before and after being subjected to the stimuli. Sometimes the changes are compared with those of a control group which has not been so stimulated. It must be said that this type of approach is socially useful but psychologically sterile: it is important to know that lectures or visits do or do not affect students but, except for sporadic attempts to correlate attitude change with obvious factors like intelligence and 'intensity' of the attitude at the outset, no effort has been made to suggest precisely why the attitudes do or do not change. They change, it is said, 'because' the student heard the lecture or met a Negro or saw a motion picture. Obviously the word 'because' is misleading. All that has been demonstrated is a correlation, not a causal connection. Two people with approxi-

mately the same attitude and subject to approximately the same stimulus pattern, for example, will be affected differently because different responses, including or excluding the attitude, are evoked within them. Or the overt effect may be the same for different reasons: in one, the afferent strength of the attitude may be altered only in respect to particular stimulus patterns and not to others; whereas in another, the drive strength of the attitude may be reduced. To maintain, therefore, that a course or a contact produces attitude change in a group of individuals (with a large or small sigma) is to state an historically limited conclusion which certainly may be socially useful at a given moment. It is like indicating that the abolition of a slum area diminishes the delinquency rate; it does, but thereby little insight is gained into the nature of delinquency (*e.g.*, why there are delinquents from non-slum areas). Research on attitude change, consequently, is likely to produce superficial generalizations which, being derived so uniquely from particular phenomena, can be applied to future situations only with extreme difficulty.

The psychological aspects of this definition of attitude, finally, can shed some light on another traditional problem: the relation between attitude and stereotype. Numerous studies, especially those of Katz and Braly (18) and of Child and Doob (10), have demonstrated the close connection between the two. The term 'stereotype' is loosely employed. Like attitude, it seems to be an implicit response—this is what Lippmann (23) many years ago must have meant by defining it as "the picture in our heads." It becomes an attitude, however, only when it is also drive-producing. An individual, for example, has a favorable attitude toward Great Britain, it is said in oversimplified fashion, because he considers the British 'patriotic.' If the oversimplified explanation be accepted as valid, some learning process in the past must be presumed. A word like 'good' must first have produced a 'favorable' response since it was conditioned to a stimulus pattern like parent or food which for other reasons evoked that response. Then 'patriotic' became associated with 'good' for reasons to be found in the individual's milieu and also evoked that response. 'Great Britain' as a stimulus came to produce the response of 'patriotic' (the stereotype), a bit of learning doubtlessly also socially conditioned. 'Patriotic' thus is a stimulus-producing response which, finally, evokes the 'favorable' response to which it has been previously linked. In this instance, the stereotype evoked by 'Great Britain' is a mediating response in a long response sequence and does evoke a drive. In like manner, most people in our society reply to the question, "Is the earth round or flat?" by means of some stereotype which produces the word 'round.' The stereotype appears to have cue- but essentially no drive-value and hence is not an attitude, although the question itself may arouse another implicit response with drive-value (attitude): to reply incorrectly to a simple question is usually non-rewarding.

5. *"An attitude is an implicit response . . . which is considered socially significant in the individual's society."* If attitudes are defined as implicit, drive-producing responses, it is obvious that they thereby are placed within the psychological sub-class of such responses but that simultaneously they are not distinguished from other responses in this group. The other psychological qualifications which have been discussed—their anticipatory and mediating character, the variety of stimuli which evoke them, and the variety of responses they evoke—are also

common to other responses which, according to conventional usage, may not be considered attitudes. The rat's aversion to a grid on which it has been shocked is doubtless implicit, it is anticipatory and mediating in avoiding the shock, it can be evoked by a gradient of stimuli, and it has drive value resulting in other responses which serve perhaps to allay its anxiety; but it is ordinarily not useful to call such a response an attitude. Similarly the human being's favorable disposition toward cream cheese or a style of hat may or may not be called an attitude. Psychologically, therefore, attitude is not and—it is deliberately maintained—should not be distinguished from the larger sub-class of responses to which it appears to belong.

The distinction between attitude and other types of responses can be made not on a psychological but on a social basis: its socially evaluated significance must be considered. The rat's aversion to the grid is not an attitude unless it plays an important part in its behavior in other situations involving rats; from the human standpoint, it is not at all socially significant. The liking of cream cheese or a hat may or may not be considered socially significant: from an overall point of view, it is doubtless trivial and hence not an attitude, although it undoubtedly is of importance to cheese producers and hat manufacturers as well as their clients. The words 'socially significant' must be left unspecific, since social values fluctuate from society to society and, within a particular society, over time and in various groups. Almost always the socially significant in a society is evaluated or, more simply, is called 'good' or 'bad' to indicate its actual or potential effect on people. A useful test to decide whether an implicit response with drive-value is an attitude is to discover whether that response is labelled desirable or undesirable by the individual's contemporaries. In any segment of society at a given moment, people are likely to acquire attitudes because they are subject to similar learning conditions.

ANALYSIS OF ATTITUDES

An ideally thorough analysis of an attitude requires knowledge (quantitative if possible) of the following factors, if its behavior and role in determining overt behavior in various situations are to be understood:

1. *Goal response:* the response pattern or patterns which the attitude anticipates and mediates and which determine its reinforcement or extinction.
2. *Perception:* the drive orienting the individual to pay attention to the stimulus pattern evoking the attitude.
3. *Afferent-habit strength:* the strength of the bond between the attitude and the evoking stimulus patterns, including the gradients of generalization and discrimination.
4. *Efferent-habit strength:* the strength of the bond between the attitude and the evoked responses, including overt ones.
5. *Drive strength:* the drive strength of the stimuli produced by the attitude.
6. *Interaction:* the strength of the other attitudes, drives, etc. with which the attitude interacts to evoke overt behavior.
7. *Social significance:* the evaluation in the society of the attitude and its direction (*e.g.*, whether positive, negative, or neutral; favorable, unfavorable, or ambivalent; friendly or unfriendly; desirable or undesirable; good or bad).

In the next section, references to these factors are placed in italics.

ILLUSTRATIVE RESEARCH ON ATTITUDES

1. *Measurement.* It has been said and perhaps demonstrated for a quar-

ter of a century that attitudes can be measured. Frequently it has been declared that an opinion is an expression of an attitude and therefore that attitude scales and polls secure a measure not of the attitude itself but of its expression in the form of an opinion. This is a simplified way of referring both to the *efferent-habit strength* of an attitude and its *interaction* with simultaneously evoked tendencies.

If the behavior of measured attitudes be analyzed, a picture like the following emerges:

a. Subjects or respondents must be motivated to cooperate. Frequently the coercive situation of the classroom provides the drive to obey the instructor, at other times there is a financial incentive, and in polling respondents may like to talk about themselves or feel flattered that their opinions are being probed. Some drive is aroused before the subject *perceives* the stimulus-pattern of the investigator and before the attitude itself is evoked.

b. Informants are then confronted with a complex arrangement of stimuli which are words on a piece of paper or from an interviewer's mouth. The perception of the stimuli elicits some kind of an implicit response: sentences, thoughts, images, stereotypes, or attitudes, all of which may not be completely conscious. One of these responses may be the attitude being investigated or may act as a stimulus to evoke it. Whether or not the attitude is evoked depends not only on whether the individual has previously learned that type of implicit response but also upon the extent to which previous stimulus patterns are similar to or different from the present pattern, *i.e.*, on earlier learning or on the gradient of generalization and discrimination (*afferent-habit strength*). Much of the research on the measurement of attitude, consequently, is an attempt to discover the various stimuli along the gradient or gradients arousing or not arousing the attitude in question. The respondent could be asked simply, "Do you like Turks?" were it not for the fact that he might falsify his reply, that he may not have previously considered all the implications of his attitude toward Turks, or that his reply could not be scaled. Instead the investigator raises questions which ask him what he has done or would do in relation to Turks. Tests involving 'social distance,' for example, suggest situations which some of the subjects probably have never seriously contemplated, like marrying a Turk or sharing a room with one: There has been no reinforcement one way or the other. The subject then generalizes from some other situation (like including a Turk or some vaguely differentiated and exotic foreigner in his club) with which he has had experience or which is meaningful to him. The gradient is from that situation to the word 'Turk.'

Guttman's (14) procedure of measuring attitudes by means of a short series of questions arranged along a continuum so that the individual's score indicates how he has answered all of them represents a very systematic attempt to determine the gradient of generalization and discrimination which a particular group reveals at a particular moment regarding a particular collection of stimulus patterns. Such a score is unambiguous only regarding the answers to the questions, but the genesis of the gradient for the individual remains unclear: The response to each question is not only a function of how closely the question approximates the original learning situation but also of the *afferent-habit strength* of the attitude, its *efferent-habit strength*, and the *interaction* occurring at the time the replies are given. The importance of previous learning and of interaction can

be seen when some of the difficulties this technique encounters are considered. Replies to a series of questions which the investigator believes elicits a particular attitude do not always assume the form of a 'scalogram'; this suggests that for some attitudes there is no consistent gradient of generalization and discrimination in the society. Guttman, moreover, works with a so-called 'coefficient of reproducibility' of 85 per cent, which means that 15 per cent of the subjects' replies do not fall within the parallelogram. These deviants of course may have misinterpreted some of the questions or have been uncooperative, but many of them probably were responding to stimulus patterns arranged along different gradients.

c. After the investigator's words have been comprehended and after, perhaps, an attitude has been evoked, the subject knows—except in the case of an open-ended question—which type of further response he can make, *i.e.*, which alternatives he may check or what he can say. What he checks or says depends on the *interaction* among all the attitudes, stereotypes, drives, etc. evoked by the investigator's words and the entire social situation. Conflict, for example, may occur between what he 'really' feels (*i.e.*, the response symbolically or actually to approach or avoid the situation in question) and what he believes is 'respectable' or what he is 'required' to write or say (*i.e.*, another implicit, drive-producing response associated with conformity or prestige or face-saving or what not); thus arises the distinction between a public and private attitude, the former of which is frequently not an attitude as here defined but an overt response to competing drives and attitudes. This interaction creates another difficulty for the attitude scale or the poll: The *afferent-habit strength* of the attitude may be such that it can be aroused by the questions but its *efferent-habit strength* or its *drive strength* may not be sufficient to affect the overt response.

d. In like manner the discrepancy between the answers which the individual gives to a written or spoken question and his subsequent behavior in the future becomes intelligible. It has been frequently pointed out that polls now possess so much prestige in the United States principally because of their ability to predict a national election. Aside from a sound approach to the problem of sampling, such accurate prediction results from a number of factors peculiar to voting and therefore cannot be considered typical or indicative of other types of polls. Except in some instances, as studies employing a secret ballot have sometimes demonstrated (2), respondents around election time do not hesitate to express their political preference to the pollster; in different words, this means that the *drive strength* of the attitude in question is greater than other interacting or competing responses. The interview and the actual voting situations are so similar that the anticipatory response when polled is likely to be aroused and to be approximately the same one eventually evoked in the polling booth (*afferent-habit strength*); and the overt response of answering the question is also similar to the overt response of voting (*efferent-habit strength*). Previously many of the respondents have told other people how they intend to vote and thus the attitude has also been reinforced. The pollster, finally, usually takes out additional insurance by polling his sample right before the election, in order to be reasonably certain that psychological changes will not occur in the interim; thus approximately the same reinforcing state of affairs continues between interview and voting.

In almost all other polling or testing situations, there are additional compli-

cations which diminish the predictive value of the replies:

(1) The question may involve a cliché which evokes an attitude not directly related to the one being measured. Since there is an overt verbal response, however, it must be assumed that something is being measured; but that something may be a function of another attitude or drive, or of a combination of drives.

(2) The question may be answered 'superficially,' which means that the stimulus pattern in the question is so different from that in the real-life situation that different implicit responses and hence overt ones are evoked. The stimulus-value of the word 'Turk' on a paper-and-pencil social-distance test is not the same as that of a flesh-and-blood Turk, even though the person may arouse the word 'Turk' which will then elicit—along with the other response already elicited—an implicit response similar to that evoked on the test.

(3) The question may be answered genuinely and the response may be a function of innumerable attitudes and drives but—because the individual may change in the interim, because he does not think things through in advance, because some of his impulses are repressed, or because he simply cannot anticipate how he will react in a future situation that is unknown to him or out of his range of experience (*cf.* 12)—the future overt response will nevertheless be a function of other drives or attitudes that are not aroused by the question.

e. Students of attitude measurement and public opinion polls have amassed evidence which indicates certainly that the replies they obtain are consistent. The same question is repeated on the same people or another comparable sample, or a split-ballot technique is employed and the results frequently remain practically the same. It could be argued that these are measures of reliability and that reliability should not be confused with validity, were it not for the fact that this verbal distinction is somewhat meaningless when applied to the problem at hand. Such consistency does demonstrate that, when confronted with a set of nearly equivalent stimuli and when other attitudes and drives within the individual remain more or less equal (as required by this technique of measurement), the implicit response affects the overt response consistently under the given conditions. It seems operationally true that some kind of attitude is being measured, but its strength and its probable influence on the individual when other drives are aroused are not thereby indicated. It is also possible that the overt verbal response can remain consistent, although in different or the same individuals it results from different attitudes and different drives.

f. The apparent fluctuation of many attitudes, which are measured by large polls, with events and changes in society indicates how weak their habit and drive strengths are. Theoretically at least, the open-ended question has a better chance of giving insight into the importance and probable stability of the attitude being measured; but its disadvantages on technical grounds are also obvious (21). In like manner, the more alternatives offered an individual, the more likely additional and relevant attitudes and drives will be aroused and the more likely it is that his overt response be a reliable guide to his future conduct. Drive and other attitudes remaining equal, one discovers how people react to words like 'capitalism' and 'communism' when they are asked to choose between the two, but this knowledge of their attitude may not be helpful in understanding or predicting their behavior when confronted with a real-

life situation involving capitalism or communism and not necessarily so labelled. It may be important to point out that people make such decisions on a superficial, semantic level. More insight would be obtained, however, concerning their actual choice in the future situation if they were made completely aware—by books, a course, motion pictures, or explicit and detailed alternatives and explanations—of what the political arrangements connoted by these terms would mean to them.

g. The principal psychological disadvantage under which attitude scales and polls labor is that, although they indirectly measure an implicit anticipatory response which is *socially significant*, they cannot determine what *goal response* the attitude now precedes. A respondent who signifies his intention to vote for a particular candidate, for example, can offer reasons for his choice and a poll can make note of these reasons. Presumably some prior contact with the candidate, his deeds, his party, or his promises (or generalization gradients therefrom, many of which are mediated by other verbal responses) has set up the anticipatory, mediating response which then is evoked literally before election time. It is the *efferent-habit strength* of the connection between those stimulus patterns and whatever the *goal response* in the individual was that is crucial and that has given rise to the attitude and hence the voting preference. Cantril and Fried (9, p. 19) make the same point when they indicate the meaninglessness of the response by people earning less than $1,000 a year to a question concerning the rate at which people earning $100,000 a year should be taxed. A crude measure of the intensity of feeling or of the importance to the respondents of the subject gives some insight, as shown by Katz (19) and by Doob (13), but this measure alone apparently does not increase predictive value in any useful way. It is a measure which does not differentiate between *afferent-habit strength* and *efferent-habit strength* nor between either of these and *drive strength*. Adequate measures of all three strengths as well as the various factors within a given personality and his society which continually reinforce or extinguish his attitude require an investigation of his life history, which scales and polls by their very nature cannot and should not be required to perform.

2. *Effect on judgment.* Social psychologists have shown experimentally that the arousal of an attitude or the interpretation of an event in one 'frame of reference' and not in another has a differential effect upon the overt behavior they call 'judgment.' This means no doubt that overt behavior is a function of *interacting* tendencies within the individual. The pioneering feeling pervading such experiments is somewhat surprising.

Over ten years ago, for example, Sherif found that judgments concerning the literary value of 16 samples of prose all taken from the same author were affected by the esteem in which the subjects held the 16 authors to whom the samples were *falsely* attributed; similar results were obtained from students in Turkey. He states that "the attitudes towards authors serve as reference points" (31, p. 374).

If the subject had seen the prose *without* the false labels, the situation might have been something like the following:

a. drive: to coöperate with the experimenter, *viz.*, to perceive the paper and to think and act.

b. stimulus pattern: the prose, the experimenter, co-acting subjects, etc.

c. implicit response sequence: verbal reactions, thoughts, attitudes, drives,

and whatever it is from past experience and in terms of momentary appreciation which *interact* to determine literary judgment.

It may be argued that the above scheme is perhaps an oversimplification of what occurs, but Sherif presents no evidence one way or the other. It is certainly clear from his work, however, that the false label introduces more complications into the reaction. The overall drive is presumably the same, the stimulus pattern is altered to include the labels, and therefore—since the label-stimuli have been previously associated with some additional response—the implicit response-sequence is different. The responses evoked by the labels are the attitudes toward the authors. If the *direction* of that response is favorable or positive, the author must be liked because contact with his words in the past has been rewarding or because the individual knows that it is fashionable (and hence rewarding in a social sense) to like such a man. As a result of this previous reinforcement, the attitude in turn must have evoked responses like "this must be good," "I like this because so-and-so is such an excellent writer," "I don't like this but I know the man has written some good books," etc. These responses apparently helped determine the overt response, the rating of the prose.

Such responses, however, were not crucial for all the individuals: Sherif's subjects displayed individual differences and his results are expressed in statistical terms. They were only relatively important in this situation, moreover, because literary judgment is notoriously subjective, which means that it results from a variety of responses whose *efferent-habit strength* fluctuates from individual to individual and within the same individual. When Birch (4) used prose passages dealing with a well-structured social point of view, he found that the attitude evoked by a false label did not always compete successfully with other stronger responses; *e.g.*, even though some of the subjects were anti-communist, many of them nevertheless agreed with statements falsely labeled 'communist' because they apparently were more strongly impressed with the arguments therein.

It is an interesting, empirical fact to know that literary judgments are affected by an author's reputation, but no more interesting nor scientifically important than the knowledge that most thirsty men want water. Sherif has noted the fact that some of his subjects deliberately weakened the drive strength of the response evoked by their attitude: in one experiment, some of them made "a special effort to overcome the influence of the authors' names" and hence their judgments were not correlated with their preferences for the authors ($r. = -.03$ for one group and $+.04$ for another). He has also pointed out that other subjects were unsuccessful in this attempt: in "written introspective records obtained right after the experiments several subjects *spontaneously* reported 'they wished that the names of the authors were not there' . . ." (31, p. 375; italics his). Why and how did these subjects make such an attempt? In addition, the fact that none of the positive correlations between authors' ranks and preferences for the prose attributed to them was higher than .53 and that at least one was as low as .30 for those "subjects who did not make a consistent special effort to overcome the influence of the authors' names or to ignore them altogether" requires an explanation in terms of the unreliability of both sets of ratings or of the *interaction* within the subjects of competing attitudes and drives. No explanation is given in Sherif's treatment

of the experiment in his book (32, pp. 122–3), nor in the work of Cantril (6).

In short, social psychologists no longer need to tax their ingenuity to devise new experiments merely to demonstrate that attitude affects judgment. This is truly an established fact, one indeed that was fairly well known even in psychophysics and before social psychologists felt impelled to blow the fresh air of what they call 'everyday life' into the laboratory. The time has come when it is possible and also necessary to relate the fact to systematic theory. Conclusions to additional experimentation should begin not with "attitude has been shown to affect judgment . . ." but with "the effect of attitude upon judgment is a function of. . . ."

3. *Effects on memory.* One section of an experiment by Levine and Murphy (22) can serve as an illustration of this effect. It has been shown by them, for example, that students who are pro-communist tend to learn more rapidly and forget less readily a piece of pro-Soviet prose than does a group of anti-communists. The authors attribute the difference to the fact that for the first group the material was, and for the second it was not, congruent with 'social attitudes.'

Under ordinary laboratory conditions, the rate of memorizing and then forgetting sense material has been found to be a function of a variety of factors related to the difficulty of the material, the number of repetitions, the spacing of the repetitions, the familiarity of the subjects with the stimuli, etc. Drive and reward are frequently neglected as factors or at least assumed to be the general, convenient ones of cooperating with the experimenter and then securing his approval. In some experiments, the strength of drive has been varied, specific rewards have been offered, or successes or failures have served as rewards. Enough is certainly known, therefore, to state the flabby generalization that drive and reward affect learning and forgetting.

The subjects of Levine and Murphy agreed to cooperate: This is the drive which is so easily aroused among college students and others and which induces them to *perceive* a stimulus pattern and otherwise respond. The prose then evoked an attitude within them, that associated with communism, because it was deliberately selected by the experimenters to fall along the gradient of other stimuli to which the response involving communism had been previously made (*afferent-habit strength*). The attitude in turn had drive value which increased the motivation of the pro-communist students and decreased that of the anti-communist group. The former may have been pleased that the experimenter was 'liberal' enough to give them communist prose and they may have anticipated that the learning of another pro-Soviet argument would be rewarding. The latter, in contrast, may have been annoyed with the experimenter for exposing them to such arguments and they may have anticipated only grief from memorizing the passage. In terms of motivation alone, then, the differential result in this experiment may be only a variation of the principle that learning is a function of reward and punishment. Other factors may have been involved, but Levine and Murphy do not provide the necessary data. The pro-communists, for example, may have been more familiar with this type of material. Or the anti-communists may have been distracted by the communist arguments and have spent some time denying their validity or refuting them.

The demonstration by the investigators of *an* effect of attitude upon learning is just another fact, in the way Sherif's experiments produce another fact. If hunger makes a rat learn to

run a maze faster than one which is not hungry, why should not a communist attitude which is drive-producing also affect the processes of learning and forgetting something about the Soviet Union? Important to note in this experiment, for example, is the fact that learning occurred regardless of attitude concerning communism and hence that the attitude affected only the rate of learning; apparently the overall drive to work with Levine and Murphy or some other drive involving individual prestige was sufficient. There were also the usual individual differences and apparently intra-individual variations.

The writers are well aware of the complexity of the problem when, in terms of what they call 'field theory,' they state:

> "The particular attitude being studied; the number and kind of subjects; their motivation; the difficulty of the material; its affective tone; the degree of conflict between the material and the attitude; the external testing situation, which includes the relations of the experimenter and the subjects; changes in the broader field from which the attitude stems—these are some of the variables that would seem to affect the way in which material which supports or contradicts our social attitudes would be learned and forgotten" (22, p. 514).

Awareness of the complexity of the problem, however, is only a commendable first step. What is also needed is a systematic theory of behavior in which these various factors can be weighted and their effects ascertained. More carefully designed experiments, moreover, will enable investigators to measure such factors.

4. *Race prejudice.* Lasker, after surveying the genesis of race prejudice among children, comes to the conclusion that, "while it would not be safe at this stage of our knowledge to attribute a relative weight to different kinds of influences, the evidence makes it probable that the attitudes unconsciously transmitted are much more effective than those deliberately taught" (20, p. 371). The kind of evidence submitted by him is illustrated by the following incident:

> "Walking across the playground, a settlement worker found a little Italian boy crying bitterly. She asked him what was the matter. 'Hit by Polish boy,' the little man repeated several times. Inquiry among the bystanders showed that the offender was not Polish at all. Turning again to her little friend, she said, 'You mean, hit by a big, naughty boy.' But he would not have it thus and went on repeating that he had been hit by a Polish boy. This struck the worker as so curious that she made inquiries about the little fellow's family. She learned that it lived in the same house with a Polish family and that the Italian mother, by constantly quarreling with her Polish neighbor, had put into the heads of her children the notion that Polish and bad were synonymous terms" (20, p. 98).

No additional data are submitted to explain this prejudice. In their absence, it must be assumed that the learning process prior to this incident involved the following steps:

a. The child overheard his mother many times describing Poles in unflattering terms and he must have seen her grow irritated by people she called 'Poles.' Gradually he learned a sentence or thought like "Poles are bad" from these stimuli. He learned the sentence or thought, as Miller and Dollard point out (25), because he wished to please his mother and to win her approval and/or because he knew from past experience that imitating her proved more often to be rewarding than punishing. At this point, he could merely respond, "Poles are bad," in response to a stimulus like "Do you like Poles?" The drive remained associated with his mother; the reward was her approval; the response to Poles was not anticipatory or drive-producing; the

child, consequently, had not yet acquired an attitude involving race prejudice.

b. It is being presumed that the child did not know the meaning of 'badness.' Gradually, as he acquired experience and more language, he learned that people and objects called 'bad' are punishing and not rewarding. This is a process of further conditioning and generalization; as a result, a response pattern of withdrawing occurred to a variety of stimuli which the child was able to label 'bad.'

c. When the child had learned that "Poles are bad" and that people called 'bad' punished him, the withdrawal response pattern previously evoked by 'bad' then could be elicited by 'Poles.' The category of Poles, in short, had become part of the secondary generalization gradient of badness to which appropriate responses were made prior to actual punishment (*afferent-habit strength*). Thereafter the sentence or person thought to be Polish produced the responses before contact had been established; and these responses in turn eventually led to overt responses relating to Poles (*efferent-habit strength*). If the child already possessed an attitude regarding people and objects called 'bad' *before* he heard his mother cast aspersions on Poles, the learning process would have been slightly different: he would again have accepted her characterization of Poles in order to win her approval and/or imitate her, but he then would immediately—without the mediating responses indicated above—have responded negatively regarding Poles even as he had been responding to all stimuli labelled 'bad.'

d. In the absence of any data from Lasker, it must be added that the acquiring of the prejudice concerning Poles might have occurred in still a different way. The generalization might not have involved the stimulus 'bad'; instead the mediating stimuli may have been those associated with anger or anxiety. The child perceived anger or anxiety in his mother whenever she mentioned 'Poles.' The sight of her reactions produced equivalent responses within him, since—again an assumption—similar behavior on her part in the past has eventually led to some kind of punishment for him. Through simple conditioning, then, the word 'Pole' came to evoke this previously acquired response pattern in him which, since it also had *social significance,* can be called an attitude.

e. This particular child for understandable reasons had difficulty in perceiving Poles. If his prejudice had been directed by his mother against Negroes or Orientals, groups with so-called 'high visibility,' his learning would have been facilitated because such groups can be discriminated on the basis of color which can be easily perceived. If his attitude toward Poles is not extinguished, he undoubtedly will gradually learn to perceive Poles as he matures; he may be able to do so on the basis of their names, the place where they live, the physical characteristics which he will come to believe they possess, they way they pronounce English, etc.

Lasker also does not indicate whether any incident similar to the one he reports had previously occurred in the life of the child. Even without this important information, however, it is possible to attempt an analysis of what must have taken place:

(1) The child was punished by another boy. This was a frustration and he felt aggressive toward the cause of his pain.

(2) The pain, aggression, anger, anxiety or whatever it was which welled up within him was a cue-producing response that mediated the response 'bad.' The 'bad' response in turn mediated

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'Pole' since it is presumed he knew a sentence like 'All Poles are bad'—the conversion of the proposition may have been a logical fallacy on his part but it was psychologically sound since the latter word fell along his gradient of generalization. Or the cue-producing response might have mediated directly the response of 'Pole' because in the past such a response within him, evoked by the sight of similar behavior on the part of his mother, had been followed or accompanied by his own response of 'Pole' (as provided by his mother or—perhaps in a sub-vocal form or as a fleeting thought—by himself). Probably there were also other implicit responses mediated by the pain, some of which—if expressed—were unsuited for the ears of the social worker. It is important to note that the child did not perceive his antagonist as a 'Pole' until he was hurt and that it was the hurting which mediated the response he gave.

(3) The social worker attempted to convince him that the boy was not a Pole but a human being who happened to be 'big' and 'naughty.' She wished to prevent the pain from mediating the response of 'Pole' and thus for the future weaken the efferent-habit strength of his attitude. Evidently the child was unable to make the discrimination for at least two reasons. In the past, firstly, both the *afferent-* and the *efferent-habit strengths* of his attitude had been heavily reinforced in his home and perhaps elsewhere; they could not be weakened by a single admonition. Then, secondly, the aggressive tendencies of the hurt child were so strong that he was in no mood to make a discrimination; *i.e.*, he sought immediate tension-reduction in a way that had brought him some relief in the past, in comparison with which drive any other drive evoked in him by the adult-status of the social worker was weaker.

(4) From a painful experience like this, it can be anticipated that the strength of the child's attitude toward Poles will be greater. He will avoid Poles or seek to hurt Poles not to please his mother—although conceivably this drive will still be aroused from time to time—but so that he himself can reduce his anxiety and avoid the punishment he might now more reasonably anticipate. The prejudice may be further reinforced by other incidents and its habit strength may be increased by the existence of similar prejudice in his milieu.

This incident has been analyzed in some detail to indicate the complex problems an apparently simple incident raises. Lasker has presented the bare externalities. Before this child's attitude toward the Poles can be changed and before race prejudice *en masse* can be tackled, the complicated factors that are present must be understood by collecting not more but the right kind of data and by analyzing them systematically.

CONCLUSION

Although tempting, it would be socially useless to conclude this analysis of the behavior of attitudes by proposing that the concept be dropped from social science. A popular word, no matter how ambiguous or unspecific, however, cannot be legislated out of existence because the fact of its popularity indicates that for the moment it is satisfying some kind of social (in this case, quasi-scientific) need. The concept of attitude is frequently accompanied by various not-too-well or clearly-formulated theories which inspire useful investigations to be conducted and thus it does discharge certain functions. In addition, it is a cozy word which laymen and social scientists think they have no difficulty in understanding: it is easier to 'intuit' the 'meaning' of a word than to search for its referents.

The telling question to ask concerning attitude is: Does the term belong in a set of systematic theories which together approximate something that might be called science? In this writer's opinion, the answer is no. Using attitude along with a few other terms does not make science but reveals polysyllabic commonsense. If the answer is no, then it follows that attitude is merely a make-shift concept which cannot stand on its own theoretical legs but requires bolstering on a more basic level and that any 'principles' pertaining to attitude are only special cases of psychological theory.

This paper has proposed that bolstering can come from general behavior theory as outlined here, and a preliminary effort has been made to relate attitude to that theory. In this way it has been possible to observe how inadequate research can be which employs the concept and to observe, too, why the research is inadequate. A theory in science, among other things, tells investigators what to look for, what to measure, and what to control. Calling an attitude "an implicit, drive-producing response" or identifying it with most of the internal responses which intervene between a stimulus pattern and a response pattern obviously does not change the phenomena to which the word refers, but it does call attention to the heterogeneous problems in human behavior. It specifies 'the other things' which the investigator of attitude has always been compelled to consider equal or has disregarded. It suggests the limited character of the results which necessarily must be anticipated when only a particular attitude—of all the tendencies affecting human beings—is studied at a given moment. It indicates, too, what the next step in attitude research might be: the application of basic psychological principles which have heretofore been derived from carefully controlled but socially limited situations.

Social scientists are interested in more than quantifying history (which is what their polls, scales, and experiments generally do accomplish). They presumably seek principles which transcend the historically limited example, not that the past or the present is unimportant but because a set of principles can set the problem for the future. Historically limited data, aside from their social utility, will always have to be collected, for only thereby can the variables affecting a given situation be weighted and predictions emerge. As behavior theory develops, it will inevitably absorb a word like attitude, perhaps in some such manner as has been indicated. Whether 'attitude' then will continue to be employed in social science is unimportant. The personal guess of the writer is that the demise of attitude in the far future will be a happy day for social science, since this event will signify the emergence of a more integrated and scientific system of human behavior.

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PSYCHOLOGICAL AND PSYCHOPHYSIOLOGICAL SIGNIFICANCE OF THE ELECTROENCEPHALOGRAM

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Electronics has presented psychology with an electrical record of our great psychological organ, the brain. Yet electroencephalography has contributed thus far surprisingly little to psychology. Electroencephalographers have possibly been so intrigued by usefulness of the EEG for diagnosis of dysfunction that attention has been diverted from its implications for normal function. Save for evidence that reaction to a stimulation is typically a 'blocking' response, a decrease of amplitude, in the electroencephalogram (EEG) (1, 8, 33, 38, 70, 77, 80, 116, 120), demonstration that this 'blocking' can be conditioned (71, 79, 115), and confirmation that something undefined but profoundly important must happen to the brain when it goes to sleep (14, 39, 64, 89), psychology has learned little new from the EEG. Evidently we either have not yet learned how to interpret the EEG psychologically or else the EEG is not a record of the integrative activities of the cortex.

Relation to integrative functions: That the EEG from the outside of the skull as usually interpreted is not a measure of the integrative activities of the cortex is evident from a number of facts: For example, imbeciles may have 'normal' electroencephalograms (11, 59). Except in comparison with conditions of extreme impairment (mental ages below 4 years) there is no significant relation between EEG and mental deficiency (81).[1] Shagass (107) found no relation to scores on a test of mental ability. EEG's may be reported normal in cortical atrophy (118), and following frontal lobotomy (43). Conversely, intellectual functions sometimes appear quite unimpaired in the presence of marked disturbance of the EEG. Andrews (3) found no 'material change' in the level of consciousness associated with 'profound' effects on cortical potential during morphine addiction. And normal intellection occurs in many individuals having practically an absence of electrical activity recordable at usual amplification. If the EEG, then, is not a measure of integrative cortical functions, what is it? Apparently it relates to some physiological activity which may be useful to, though not actually essential to the psychologically adaptive functions. Since changes during sleep and excitation indicate a possible relation of EEG to the adaptive mobilization of the organism, the EEG in this respect appears similar to other preparatory and facilitative bodily changes which have been studied as indicators of psychological arousal and reaction. That it is like these processes in contributing to homeostatic regulation will be shown by available evidence (29–38). Psychological implications of over- and under-regulation involving the EEG will be elaborated.

EEG frequency and potential: Cerebral function involves the three types of EEG activity, alpha, beta, and delta,

[1] Knott, Freedman & Bardsley (78) report for 12-year-olds a correlation of .12 with EEG frequency and of .08 with alpha index. The implication of their .50 and .23 correlations, at the 8-year level, and Henry's similar results (65) at 10-year and lower levels is uncertain as Henry points out owing to the expected relation of EEG at those ages to varying degrees of physiological maturation.

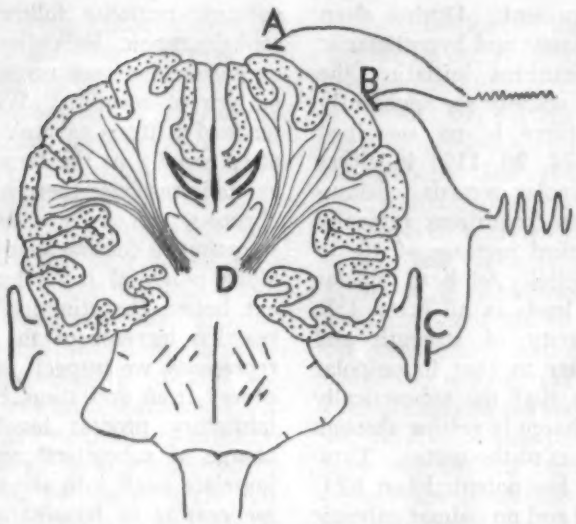
which are doubtless familiar (59, 70, 80, 84). Delta slow waves are found in normal persons primarily in sleep; they are also pathognomonic of many abnormal conditions. Alpha waves occur with greatest prominence in conditions of waking relaxation, decreasing either as a person goes to sleep or as mental activity is increased (38, 52). They range from 7 to 14 per second, depending primarily on the age (65, 86, 108) and metabolic level (56, 67, 72, 87, 101, 104, 111); they are the highest normal waking potentials recordable from the outside of the skull; and they tend to be strongest in occipital regions (1, 2, 65, 71, 72, 85, 103). Beta low potential fast activity from 18 to 40 per second, tends to be more prominent in frontal regions (71, 72, 103) and typically displaces waking alpha activity during increased 'tension' or excitation (9, 16, 33, 52, 62, 75, 76, 88, 110, 112). Electrical activity of the waking state thus presents a paradox, low potential beta being associated with maximum cerebral activity, whereas high potential alpha characterizes a low level of waking cerebral activity (29). The familiar 'blocking' of the alpha rhythm by stimulation illustrates the relation.

Alpha and activity: Why should the most prominent electrical potentials of the brain be decreased when cerebral activity is presumably increased? In other bodily organs such as the heart or muscles electrical activity increases with increase of function.

A possible explanation of this paradox is apparent, as we understand the nature of the alpha rhythm. It has been demonstrated by undercutting the cortex, by recording at various depths within the brain, and by destruction in the thalamus (7, 12, 13, 45, 46, 47, 69, 92, 93) that the 10 ± per second rhythm recorded from the outside of the skull depends upon impulses reaching the cortex over thalamo-cortical pathways. It is explained that these rhythmic impulses cause the cells of the cortex to beat in unison and that what is recorded as the normal alpha rhythm from the outside of the skull is the electrical effect of great masses of neurones beating in unison. The cortex in turn sends rhythmic impulses back to the thalamus (12, 13, 46, 47, 69) and thus provides a resonating feedback circuit between thalamus and cortex which helps explain the large rhythmic potential differences recorded from the outside of the skull.

Under conditions of cortical excitation, on the other hand, whether the excitation is due to external stimuli or to self-maintained processes in the brain, the cellular elements, particularly those in the fine meshwork constituting the three outer layers of the cortex (10, 11), take up their own individual rates of discharge, potential differences tend to cancel one another out, and the rhythmic 10 ± per second electrical activity is decreased (7). Only low potential fast activity remains—and may be registered if the recording is adequate (49). 'Blocking' of the alpha rhythm involving fast activity of this type is perhaps most consistently prominent in frontal-motor leads during the latent period preceding the palmar galvanic response (38). Such records conceivably represent intercortical potential differences. It is apparently the low potential fast activity not the alpha rhythm which corresponds to the integrative processes of the cortex.

Bipolar indications of cortical activity: In the normally active brain this low potential fast activity is most characteristic of 'bipolar' EEG records between frontal-motor and motor-parietal (but not parietal-occipital) electrodes on the scalp. Such bipolar records are possibly more definitely indicative of activity within the cortex than uni-



polar [2] (to ear lobe) records from the same region. When one considers the relation of EEG electrodes on scalp and ear lobes to the thalamocortical pathways, as illustrated in the figure, it is obvious that the thalamocortical potential differences between B and D would be readily picked up by connections between scalp, B, and ear lobes, C, which include (as by a potentiometer) part of the thalamocortical circuit. Thalamocortical potential differences would not so readily be picked up by bipolar (intracortical?) connections between A and B which are at right angles to the projection system B–D. Potential differences between points on the cortex, on the other hand, would be best picked up by the bipolar (intracortical?) leads, A–B, on the scalp.

Bipolar connections between parietal and occipital leads provide a better pick-up of thalamocortical potential differences than more frontal bipolar leads possibly on account of the curvature of the skull. Owing to the shifting of the plane of the scalp from horizontal in front to vertical in the occipital region, parietal-occipital bipolar connections toward the back of the head, like the 'monopolar' connections to the ear, subtend the arc followed by the thalamocortical optic radiations. Occipital bipolar, like occipital unipolar (to ear lobes) records may accordingly be greatly influenced by thalamocortical potential differences. The prominence of the bipolar parietal-occipital alpha rhythm may in part be thus accounted for.

Description of actual patterns of response may clarify relationships (38). A drowsy or sleeping state is typically characterized by a flat or slow electroencephalographic record (14, 39, 64). Occasional bursts of 14-per-second ac-

[2] Strictly speaking there is no such things as a 'unipolar' record. The term indicates only that one of the two recording electrodes is over a cortical area and the other not. That a connection to the ear lobes, for example, provides a 'diffuse' conection is only *relatively* true, for likewise an electrode on the scalp recording the relatively high potentials attributable to summation effects in large masses of neurones, is also acting as a 'diffuse' electrode. Sinusoidal positive and negative swings of potential such as those seen in the 'unipolar' alpha rhythm may simply indicate that groups of cells in the cortex and in some distant region nearer to the indifferent electrode are discharging alternately.

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tivity may be present. During sleep the essential thalamic and hypothalamic (53, 102) mechanisms initiating the 10 ±-per-second activity are apparently quiescent and there is no sustained alpha rhythm (74, 96, 119) in either monopolar or bipolar records. Stimulation under these conditions typically causes brief marked increase of 10- to 14-per-second activity of both bipolar and monopolar leads in all areas (38, 89), the similarity of strength and pattern in bipolar to that in unipolar leads suggesting that the subcortically determined discharge is getting through to the outer layers of the cortex. Typically no bipolar low potential fast EEG activity appears and no palmar galvanic response occurs at this stage. A strong alpha rhythm may persist for some time following arousal. It seems likely that a mobilization of subcortical pacemaker mechanisms has occurred which, in the relative absence of cortical activity, momentarily dominates the cortex. Such a pattern, from the psychological point of view, possibly indicates an arousal without orientation.

If, following awakening with initiation of the alpha rhythm, a subsequent stimulus is presented it may be followed by the typical 'blocking' of the alpha rhythm by the low potential fast EEG activity in frontal and motor regions, and by a palmar galvanic response typical of attention and orientation (25, 38, 70). The bipolar frontal-motor fast activity typically begins during the latent period of the galvanic reaction. This suggests that excitation reaches the frontal and motor cortex, affects cortical sympathetic centers (26), and discharges thence into the peripheral sympathetic system to cause the galvanic reaction. In an earlier study both increase of fast 'beta' and decrease of alpha 10 ± per second activity were correlated with galvanic change (33).

However, the appearance of a palmar galvanic response following electroencephalographic indications of cortical excitation does not necessarily end the pattern of reaction. With the occurrence of a strong galvanic deflection and often exactly at the termination of the galvanic response there is frequently an abrupt return of the alpha rhythm, even sometimes a definite augmentation of the alpha potential beyond anything present before the stimulus.[2a] This post-reaction increase of the alpha rhythm represents, we suspect, not merely a recovery from excitation, but actually an inhibitory process involving the discharge of subcortical and sympathetic impulses back into the cortex, thereby moderating or terminating the cortical excitation. Williams (120, 121) has noted consequent relaxation of the subject. Both the moderating effects of the 10 ± per second rhythm on cortical activity and the constrictor effects (5, 6, 30, 33, 82, 109) of sympathetic activity or of inhibition of parasympathetic activity on cerebral blood supply may be involved. Furthermore the sympathetic system may itself carry a 10 ± per second rhythm (18, 19, 97, 98). The possible moderating influence of the sympathetic system is indicated by Murphy and Gellhorn's (94, 95) demonstration of marked increase in cortical excitability following cutting of the sympathetic pathways.

It is an old story that depicts a psychological stimulus in the brain as setting off impulses to the autonomic system which produce peripheral preparatory and facilitative changes such as palmar sweating and blood pressure (25). Familiar also is the concept that activity in the brain gives rise to chemical, autonomic, and vasomotor changes which facilitate cerebral activity (28, 29, 33, 54). That the process, however, also works the other way around, and that chemical circulatory, autonomic

[2a] Often most marked frontally.

and thalamo-cortico-thalamic resonant activity may moderate and regulate cortical integrative activity has not been so generally emphasized, or possibly has not been identified for what it is.

Need for regulation of the cortex: The organization of the three outer layers of the cortex is apparently such that, in the absence of sufficient regulation, activity in the intricate meshwork of fine fibers and small ganglion cells, once started, may become excessive or self perpetuating. Status epilepticus (44) may represent such a self-maintained activity. States of excessive anxiety or 'tension' are possibly conditions where self perpetuating circular, 'ruminative' processes pass beyond the patient's control. In anxious and schizophrenic persons such self-maintained activity (and low potential EEG's [9, 15, 16, 34, 41, 42, 43, 73, 87, 88, 105, 110]) may persist to the point of exhaustion.[3]

The explanation that neuronal elements in the cortex during excitation or anxiety are discharging irregularly and rapidly, their potentials cancelling one another out and 'jamming' the 10 ± per second wave from the thalamus, may account for the low potential fast activity. But this is only half the story. Why is the individual tense or anxious and his cortex so highly active? Can it be assumed that the activity in the outer layers of the cortex, particularly in frontal regions is necessarily primary in the initiation of such tension? Or is it possible that in low threshold conditions favoring circular and self-perpetuating activity in the neurones of the cortical meshwork the regulation is inadequate? The excessive fast activity in the cortex which is more or less typical of conditions of anxiety or 'tension' (9, 15, 16, 34, 75, 105) doubtless represents a condition of high metabolic activity (56, 67, 87, 104) associated with marked production of CO_2 and other metabolites. The resulting intracellular chemical condition may favor increased synthesis of acetylcholine (23, 48, 114) or its conservation from the destructive action of cholinesterase (57, 99), and acetylcholine is known to increase cortical excitability, and even to produce convulsive activity (17, 20, 22, 50, 91). Fast activity in parasympathetic nerves (4, 32, 100), including that in nerves from the carotid sinus (20, 51), increases the liberation of acetylcholine and may contribute to cortical 'tension.'

The opposite condition involving high potential and slow waves requires a different form of moderation and regulation (30). Such slow high potential patterns in the EEG are possibly seen most frequently following hyperventilation with blowing off carbon dioxide from the blood (39, 58).[4] The increase of alkalinity with increased destruction of acetylcholine by cholinesterase under these conditions (57, 99) may greatly slow down cerebral activity and, possibly as a consequence of loss of vasodilator substance, result in spasm of cerebral bloodvessels. The condition is particularly acute during the inhibition of parasympathetic activity indicated by an increase of heart rate (5, 6, 35, 36) when the loss of acetylcholine in the brain ceases to be reimbursed by the parasympathetic nerves. Under these conditions carbon dioxide or a drug such a physostigmine, which conserves acetylcholine, will, in moderation, maintain more normal activity (31). Individual differences in EEG relating to

[3] Various forms of shock treatment of such patients may involve either reduction of cortical metabolism (66) or increase of sympathetic activity (54) or both. Increase of alpha activity (15, 83) may be thus accounted for.

[4] That this ventilation effect is small in schizophrenic patients according to Rubin and Turner (106) is of interest relative to observations on acetylcholine in the preceding paragraph. Also see Gellhorn *et al.* (54, 55) on parasympathetic activity in schizophrenia.

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varying individual levels of autonomic activity have been reported (37, 63).

A normal alpha rhythm thus apparently requires that there be neither great excess nor great deficiency in conditions favoring cortical activity. And just as a state of waking relaxation is a condition favorable to strong palmar galvanic responses to stimulation (27), so a well developed alpha rhythm indicating absence of excessive tension may be a condition favoring observable 'blocking' by stimulation.

Several lines of evidence indicate that the 10 ±-per-second activity is itself an important part of the homeostatic regulating mechanism which maintains and moderates physiological conditions of cerebral activity (28, 29, 33, 54). As by a radar system, the thalamus broadcasts its 10 ±-per-second wave, which, depending on the state of the cortical receptor, may or may not be reflected back on the thalamus and affect its activity. And, depending upon the amount of activity already going on in cortical structures, the thalamus may or may not be able to impose its frequency and thereby exercise a regulatory control.

That this 10 ±-per-second activity of the brain itself serves a regulatory function may be inferred from several considerations (29, 38). Certainly a sufficiently strong subcortical 10 ±-per-second rhythm might be able so to pace activity in the cells of the cortex that they could not carry on individual activity but must succumb to the 10 ±-per-second regimentation. The mere fact that the alpha rhythm involves a strong positive potential swing every tenth of a second would, by the known relation of anodal (positive) activity to neurone function, tend regularly to interrupt ongoing activity in the cortex. Thus decrease of alpha rhythm may not only be a consequence of mental activity or tension, but a deficiency of alpha rhythm may be a factor permitting increase of 'tension.' That there is need for such regulation, and that both over- and under-regulation may contribute to pathological conditions has not perhaps been sufficiently recognized.

Emotion and over-regulation of the cortex: Contrasted with conditions of 'tension' due to deficient regulation of cortical activity is the possibility of an imbalance in the opposite direction. Instead of the inadequate emotional and autonomic reactions which may characterize a maintained tension there may be an excessive peripheral activity associated with apparent limitation of cortical function. Meduna (90) has noted a contrast and incompatibility between conditions of schizophrenia and epilepsy. Jasper, Fitzpatrick and Solomon (73) find some confirmation of this view in the difference between electroencephalographic patterns of these groups. Greater high potential and more slow activity may be found among the epileptics. And in conditions of bodily hyperactivity (as contrasted with conditions of tension) there is frequently observed a marked increase of alpha potential. Instead of the typical 'blocking' of the EEG accompanying cortical excitation, increased alpha and slower EEG potentials following stimulation may indicate a reduction of cortical activity. A 'relative functional decortication' (24) may be involved.

For example, Thiesen (112) gives an instance where fear of a white rat led in a young girl to increase of alpha and a marked decrease of beta activity. Travis and Malamud (117) report such facilitation effects in stuttering. Lemere (87) presents similar observations on the effects of drugs and stimulation. Grinker and Serota (60, 61), and Hoagland, Cameron and Rubin (68) report effects of emotion on high potential slower waves (see 110). Knott (76) gives an illustration of a facilitation re-

action in emotion involving both increase of amplitude and frequency. Williams (120, 121) notes the frequency with which the effect occurs in conditions of anticipation following presentation of an expected stimulus. Conditions where increase of cortical activity accurs during the anticipation of a familiar stimulus, and where the giving of the stimulus relieves central 'tension' and mobilizes the peripheral reaction mechanisms, may produce this effect.

If we may discount the probability that some primary intrinsic mechanism in the cortex can reduce cortical activity in the presence of emotional excitation we still have several mechanisms by which the effect could occur secondarily. Increased rate and depth of respiration during emotion may wash out the CO_2 in the blood (30, 31, 36), and increased blood pressure with increased blood flow during emotion may reduce the CO_2 of the brain (33, 35). This would result in greater alkalinity and greater destruction of acetylcholine by cholinesterase. Inhibition of parasympathetic activity with increased heart rate (30, 31, 35) likewise may reduce the liberation of acetylcholine in tissue surrounding the cerebral blood vessels and contribute thereby to a deficit of acetylcholine and possible impairment of nerve conduction. Decrease of vasodilator CO_2, decrease of acetylcholine and increase of sympathetic impulses to the brain may all favor constriction or even spasms in cerebral blood vessels, thereby embarrassing the cerebral blood supply and restricting essential oxygen (40, 56). Cerebral constriction typically occurs in conditions favoring increased alpha potential (29–38). Increased subcortical activity with increased output from mechanisms pacing the alpha rhythm might conceivably also occur. All of these influences which might under normal conditions serve to regulate and prevent excessive and self perpetuating cortical activity would, in excess, (a) reduce or block cortical activity and (b) increase alpha and activity of slower EEG frequencies. Some of the EEG changes in emotion and certain instances of so-called 'psychomotor' attack may thus be accounted for. Again it is obvious that what is picked up from the brain by the EEG depends upon the relative strength of the cortical fast activity as compared with slower subcortically determined activity which, to varying degrees, is being impressed on the cortex.

Evaluation: Insofar as these mechanisms permit us to arrive at a correct psychological and psychophysiological interpretation of the EEG, possibilities for electroencephalographic contribution to psychology are increased. Indices of cerebral function may be provided by which to gauge the course of psychological experiment. On combining EEG with other physiological measures we may find new criteria for the evaluation of mental states with respect to such things as sleep, 'tension,' attention, and the relative contribution by cortical and subcortical mechanisms in emotion. We shall have a rationale for deliberate modification of the observed psychological states and for the evaluation of the success of treatments such as psychotherapy, chemotherapy, relaxation, etc. Understanding of the electroencephalogram, when obtained, will not only give new insight into mechanisms of psychological function but provide clues as to what can be done to change and study it.

SUMMARY AND CONCLUSIONS

Failure of electroencephalography to make important and extensive contributions in the field of psychology is inferred to indicate either (1) that we have not understood the psychophysiological implications of the electroen-

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cephalogram or (2) that the more prominent features of the EEG do not provide a record of the integrative activity of cortex. That the EEG is not primarily a record of the integrative activities but of facilitative and homeostatic regulatory processes which contribute to but which are not essential to integrative cerebral function is shown from the available evidence. It is shown that the activity recorded by the electroencephalogram is part of a mechanism of regulation which maintains varying degrees of equilibrium among the four variables, alpha rhythm, cortical integrative activity, chemical condition and cerebral circulation, with the autonomic nerves to the brain providing an important contributing influence. It is shown how each of these affects each of the others and how an imbalance in the direction of either under- or over-regulation may give rise to psychopathological effects. Implications regarding possible mechanisms of 'tension,' anxiety, schizophrenia and emotional 'functional decortication' are considered with respect to frequently observed effects on the electroencephalogram. Applications in psychology are considered.

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NEURAL STRUCTURE IN PERCEPTION AND RESPONSE [1]

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1. *Introduction.* The purpose of this note is to exhibit hypothetical nervous nets capable of 'explaining' certain perceptual processes. The explanation sought is formal merely, and in no sense metaphysical. It is taken to be self-evident that a formal explanation is the most that science can yield.

There are psychologists who tend to argue that perception and behavior are not explainable in structural terms exclusively. The argument is necessarily vague. One can, of course, so delimit those theories which one chooses to designate as structural that particular processes are demonstrably unexplainable in such terms. For the most part the particular processes chosen for discussion here are among those sometimes adduced as unexplainable structurally.

2. *Basic postulates.* Before attempting to set up our structures we must have suitable hypotheses as to the properties of the constituent elements, the neurons. We employ for the structures to be set up, the hypotheses of McCulloch and Pitts (5) as follows (for a somewhat more elementary exposition *cf.* 1): Let the life of the organism be subdivided into short time-intervals of length about half a millisecond. This is the approximate length of the synaptic delay and is also roughly the length of the absolute refractory period. Any neuron which fires once during one of these intervals cannot fire again until the next. It will fire only in the interval following one in which a sufficient number of impulses impinge upon it via endfeet from neurons afferent to it, and provided there was no concurrent impulse in a neuron forming an inhibitory connection with it. In the diagram we shall represent endfeet making excitatory connections by solid circles; endfeet forming inhibitory connections are represented by encircling the triangle which denotes the cell body. In these diagrams, two concurrent pulses at excitatory endings are supposed to be required for stimulation. Thus in Fig. 1, the neuron N_2 connects by two excitatory endfeet with N_4 as indicated by the two solid circles against N_4. Hence the activity of N_2 in one interval is sufficient to secure the activity of N_4 in the next interval of time. Likewise, in the same figure, the activity of N_1

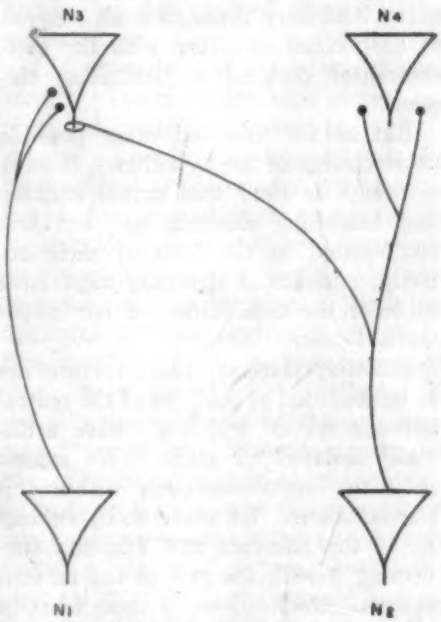

FIG. 1. Discrimination

[1] The opinions or assertions contained herein are the private ones of the author and are not to be construed as official or reflecting the views of the Navy Department or the naval service at large.

would secure the subsequent activity of N_3, but only if N_2 does not fire at the same time as N_1. This is because of the inhibitory ending from N_2 upon N_3, represented by the open circle about N_3. In Fig. 3, N_0 and N_1 must act concomitantly to secure the activity of N_2.

We shall not undertake to argue the legitimacy of these assumptions, though a remark about the use of inhibition may be in order. The fact that there must occur inhibitions of nervous activity is not subject to question. One may question whether the inhibitors are themselves nervous, or whether, if they are neurons, the effect is a function of dynamics (Wedensky), structure, or whatnot. But the assumptions made here are the simplest possible, whether or not one may wish to substitute for every inhibitory connection an element of unspecified structure with the noncommittal designation 'inhibiting element.'

But as for this and other possible shortcomings of the hypotheses, it must certainly be clear that actual exciting and inhibiting elements are not less complicated in the laws of their activity, and actual structure must have at least the capabilities of our hypothetical ones.

3. *Interpretations.* Our structures are to be regarded as portions of the central nervous system which we have artificially isolated for study. We exhibit only the interconnections within this portion alone. We make no hypotheses as to the afferents and efferents connecting it with the rest of the nervous system. Unquestionably these must be numerous. Within the portion which we do exhibit, there are to be distinguished those which we shall designate as the cells of the sensory level; those cells which we shall designate as the perceptual or the effector elements; and the internuncial cells which go to make up the connecting net, conducting or inhibiting the transmission of impulses from cells of the one group to cells of the other. In applying the terms 'sensory,' and 'perceptual' or 'effector,' we refer only to the relative positions of the cells so designated within the structure under consideration. We suppose only that the spatio-temporal configuration of the impulses in the sensory elements is directly representative of the spatio-temporal configuration of the proximal stimulus, and analogously that the configuration of impulses in the perceptual elements (or the effector elements) is directly representative of the perception (or the motor response).

In the mechanisms presented here no afferents to cells of the sensory level are exhibited; no efferents from cells of the perceptual or motor level are exhibited. Hence, the internuncials, and these alone, have both afferents and efferents within the net.

4. *Discrimination.* For purposes of later reference we present first the simplest possible discriminating mechanism (in this connection, *cf.*, 6). We understand the discrimination of two stimulating intensities to mean the qualitative differentiation of the response. Thus suppose a stimulus of intensity R_1 evokes a characteristic response S_1, while a stimulus of greater intensity R_2 (but of the same kind) evokes a characteristic response S_2. If S_1 and S_2 differ only in magnitude or intensity (S_2 involving all muscle-groups involved in S_1 and also others beside) this does not constitute the discrimination in question here. But if S_1 involves muscle-groups not involved in S_2, then there is a qualitative difference between the responses (whereas between the stimuli there was assumed to be only a quantitative difference). This is the form of discrimination that seems to require explanation.

To be concrete we may imagine the stimuli to be two degrees of warmth,

the lesser degree R_1 evoking an approach, S_1, the greater degree R_2 evoking avoidance, S_2. It is reasonable to suppose that the thermal receptors may differ as to threshold. In Fig. 1 let N_1 and N_2 represent two neurons at the sensory level with respect to the mechanism. Whether these are actual thermal receptors, or are merely efferent from the thermal receptors is of no consequence at the moment. We assume only that the weaker stimulus R_1 causes, directly or indirectly, the firing of N_1 but not of N_2, whereas the stronger stimulus R_2 causes, directly or indirectly, the firing of both N_1 and N_2.

The neurons N_3 and N_4 in this mechanism lie at the motor level with respect to the mechanism. That is, N_3 and N_4 are more or less directly afferent to certain muscle groups, N_3 to those involved in an approach, N_4 to those involved in an avoidance response. It is not necessary to assume that N_3 is connected to all and only the muscles involved in approach, nor that the connection is immediate. It is enough to suppose that N_3 is representative of neurons which lead, perhaps through chains of efferents in series, to muscles involved in the reaction.

To account for discrimination of the type under discussion, we suppose that N_2 forms an inhibitory connection (the open circle in the figure) with N_3. Then the occurrence of R_1 leads to the activity of N_1 but not N_2; the activity of N_1 leads to the firing of N_3; this in turn, leads ultimately to the occurrence of the response S_1. On the other hand, when the stronger stimulus R_2 is present, both N_1 and N_2 fire. The inhibition from N_2 prevents the firing of N_3 in spite of the activity of N_1, so that those muscles are at rest that are involved in the response S_1. On the other hand, because N_2 is firing, N_4 will fire, and this, in turn, leads to the appearance of the response S_2.

Formally, at least, and on the basis of the McCulloch-Pitts postulates, this mechanism fulfills the requirements for a discriminating mechanism allowing two stages. A thermal stimulus of intensity sufficient to activate the sensory neuron N_1, but not sufficient to activate the sensory neuron N_2, will evoke, by this mechanism, an approach response S_1; a thermal stimulus of intensity sufficient to activate N_2 will evoke, by this mechanism, the qualitatively distinct response S_2 of avoidance. It is clear that any other modality might have been used—brightness, pain, pressure, or whatever—and with these the appropriate pairs of responses. Moreover, we could easily account for any number of discriminable ranges and associated responses by including an equal number of sensory and of motor elements.

5. *Constancy, and the failure of constancy.* The first structure of real interest relates to the constancies. 'Constancy,' as we here understand it, may be described as follows (*cf.*, in particular, 4 for an elaborate discussion of constancies in various forms): Suppose an object is placed in varying relations to a subject who perceives it (varying distances, varying degrees of illumination, varying aspects, etc.), and suppose that among the physical attributes of the stimulus field one of them is perceived under two different aspects as two distinct attributes of the object. Of the two attributes of the object, one may be an invariant attribute of the object (*e.g.*, its size), independent of its physical relation to the subject and to the conditions in its environment, the other being a relation of the object to its environment (*e.g.*, its distance from the observer). According to the circumstances, a change in the physical attribute of the stimulus field may be perceived as a change in this relation alone, the perception of the invariant

attribute of the object remaining constant, or nearly so. Or the change in the stimulus may be perceived instead as a change in the invariant but not in the relation. Or again, by merely changing the surround, both the invariant and the relation may seem to change—both the size and the distance. The first of these effects is constancy as such—*e.g.*, when an object retains its apparent size despite its varying distance, or a screen retains its apparent color despite changes in the illumination. True constancy is the constancy of the perception of the invariant attribute of the object (*e.g.*, its size) despite the change in the stimulus field (the retinal image). But it is important to note that the constancy is seldom absolute, and when it is, it is only one aspect of a more general linkage of the perception of the invariant attribute with that of the relation of the object to its surround. Our mechanism allows for either constancy or the failure of it, according to the conditions in the surround.

The case of whiteness constancy is somewhat easier to analyze. In this the object is an illuminated gray paper; the invariant attribute is its albedo and the relation in question is the degree of illumination. The inflow of energy in the form of light is the physical attribute of the stimulus field. Any given albedo or shade of gray W_1, illuminated with a given intensity B_1 will produce an associated rate of inflow of light energy per unit area of the retina. However, if a paper with higher albedo W_2 were used, a somewhat lesser intensity B_2 could yield the same rate of inflow of light energy. As the illumination varies the perception of W_1 may remain unchanged and 'correct'; an arrangement of deceptive cues in the surround, however, may induce an erroneous perception of W_2 instead of the correct perception of W_1, but then associated with the erroneous perception of W_2 will be an erroneous perception of B_2, the degree of illumination. Speaking crudely, if the paper can be made to appear as part of a region of uniformly low illumination it will appear then to have a high albedo or reflecting power; but if it appears as part of a region with uniformly high illumination its albedo will appear to be low. The conditions under which it may be made to appear as highly or slightly illuminated for a given intensity of the proximal stimulus due to the paper, are experimentally determinable, and are not matters of present interest. The points of concern are, first, that they are external to the paper and its retinal image, and second, that for a fixed absolute brightness the apparent albedo and the apparent illumination tend to vary inversely with changes in the surround.

Now the optic tract certainly does not consist of nonintersecting sequences of neurons stretching each from a rod or cone to a definite point in the striate area. On the contrary, there are horizontal cells even in the retina, and throughout the tract the paths converge and branch in an apparently hopeless confusion. It is therefore not at all surprising that the visual perception of an object may be altered by a change in the surrounding field, even while the stimulation due to this object itself stays constant.

Our problem will be the following: Consider any single retinal element. When it is stimulated by a given intensity, then according to the conditions (*i.e.*, according to the distribution of the stimulation at other points of the retina) the resultant perception can be of a gray in one of a number of shades (albedos) W_1, W_2, . . . , with an illumination in one of a number of degrees of brightness B_1, B_2, ... When the intensity of stimulation of this retinal

element is held fixed, then associated with any resultant perceived shade W, there will be just one perceived illumination B, and conversely. Hence if the perceived grayness darkens, the perceived illumination brightens.

Consider one of the retinal elements affected by the image of the paper. Suppose the transmitted nervous impulses, after traversing a discriminating net such as is represented in Fig. 1, reach and activate a certain neuron N_0. This shall be the sensory element for the net to be considered now. To simplify the situation, let us consider two possible shades only of gray, W_1 and W_2, with W_1 the darker, and the two degrees of apparent illumination, B_1 and B_2, which are associated by the particular intensity of stimulation employed. Let the neurons N_1 and N_2 mediate the perception of the two degrees of illumination, while N_1' and N_2' mediate the perception of the albedos. Our problem is merely to provide a structure by which the activity of N_0 may occasion the activity of both N_1 and N_1', or of N_2 and N_2', but not the concomitant activity of any other combination, and not the activity of any one alone. When the activity occurs in N_1 and N_1', and when in some other associated pair, we do not attempt to explain. For our problem the conditions are fulfilled quite simply by the structure represented in Fig. 2. One internuncial neuron connects N_0 to both N_1 and N_1', another to both N_2 and N_2', while the internuncials themselves are mutually inhibiting. We have further included afferent inhibitors from the outside upon the internuncials to represent the influence of the surrounding field. It is assumed that one or the other, but not both, will carry impulses preventing the activity of the efferent internuncial.

By this mechanism N_0 can arouse activity in both internuncials, that leading to N_1 and N_1' as well as that leading to N_2 and N_2', while activity in either internuncial will cause the firing of both its associated perceptual neurons. However, the mutual inhibition of each internuncial by the other prevents both from firing together (except perhaps for a single initial pulse), and the afferent inhibition from outside the net, the postulated effect of the surround, we are supposing to inhibit one or the other, but not both, of the internuncials. Thus with a fixed absolute brightness of the object sufficient to activate N_0, the perceived illumination and albedo will be either as mediated by N_1 and N_1', or else as mediated by N_2 and N_2', according to the perceived conditions in the surround.

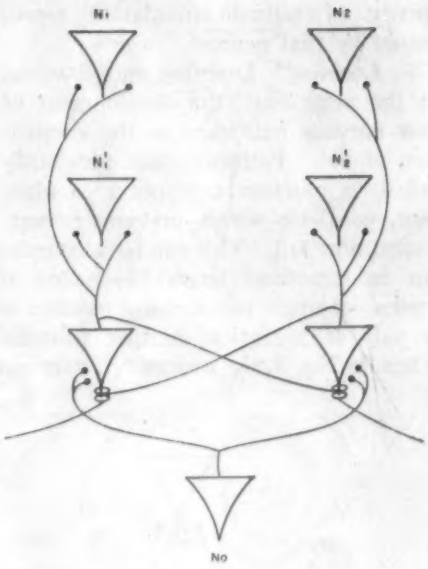

FIG. 2. Constancy

Other degrees of absolute brightness will cause activity in other sensory neurons (not shown in the figure) besides N_0, and each will be connected, through internuncials to the perceptual neurons of the two series paired in a different way. The pairings will accord for each sensory neuron with the particular in-

tensity of retinal stimulation represented by that neuron.

6. *Learning.* Learning must involve, at the very least, the development of new nervous pathways or the elimination of old. Pathways that previously failed to function now begin to function, or those which previously functioned now fail. This can be accounted for in structural terms by means of cycles—a single self-exciting neuron, or a pair of mutually exciting neurons. Thus, in Fig. 3, the neuron N_1, does not

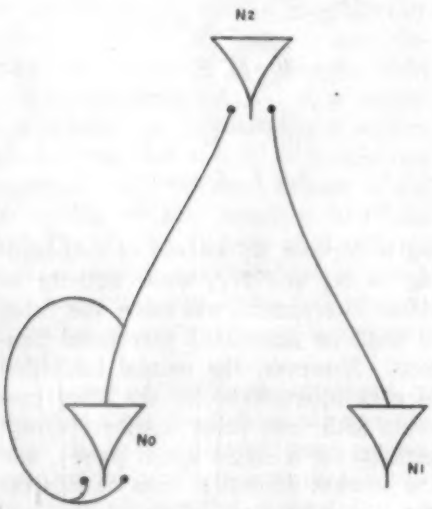

FIG. 3. A "learning" cycle

alone activate N_2 since it sends only a single endfoot to N_2. But if the cycle formed by N_0 is somehow activated, it remains perpetually active, and its bombardment of N_2, though subliminal by itself, will be sufficient to summate with an impulse arriving from N_1 so as to cause N_2 to fire. Hence the pathway through N_1 would function only when N_0 is active, and physiologically learning might consist in the activation of such cycles as N_0. Where the learning involves the elimination of pathways, we may imagine such structures as that of Fig. 3, where, however, N_1 sends two endfeet to N_2 and N_0 makes inhibitory connections with N_2.

Circular structures of this type are quite common in the nervous system and various hypotheses have been set forth in attempts to explain their function. If learning is in fact their function, it is very likely true that not one, but a large number of such cycles are involved in any given learning process.

7. *Transposition.* In this account our interest lies not so much in learning as such, but in a particular phenomenon associated with learning, commonly known as transposition (*cf.*, 2 for a very extensive investigation of this phenomenon). Having shown that the modifiability of pathways can be understood in structural terms, we may assume this in our description of transposition.

Suppose an animal has been trained to react positively to, let us say, a certain shade of gray, and negatively to a darker shade, whenever the two shades are presented together. Let B designate the brighter, C the darker shade. Then in general, if B is paired with a still brighter shade A, the animal will respond negatively to B (to which he had formerly reacted positively), and positively to A. Similarly, if C is paired with a darker shade D, the animal now reacts positively to C (to which he formerly reacted negatively), and negatively to D. More concretely, let us suppose that as a result of his training, the animal jumps to the left if B is on the left while C is on the right; but he jumps to the right if B is on the left and A on the right.

Consider the structure of Fig. 4. Suppose the motor element N_L is connected with the effectors involved in a jump to the left, and N_R with the effectors involved in a jump to the right. Suppose the thresholds of the sensory elements N_{L1} and of N_{R1} are equal and low; those of N_{L2} and of N_{R2} are equal

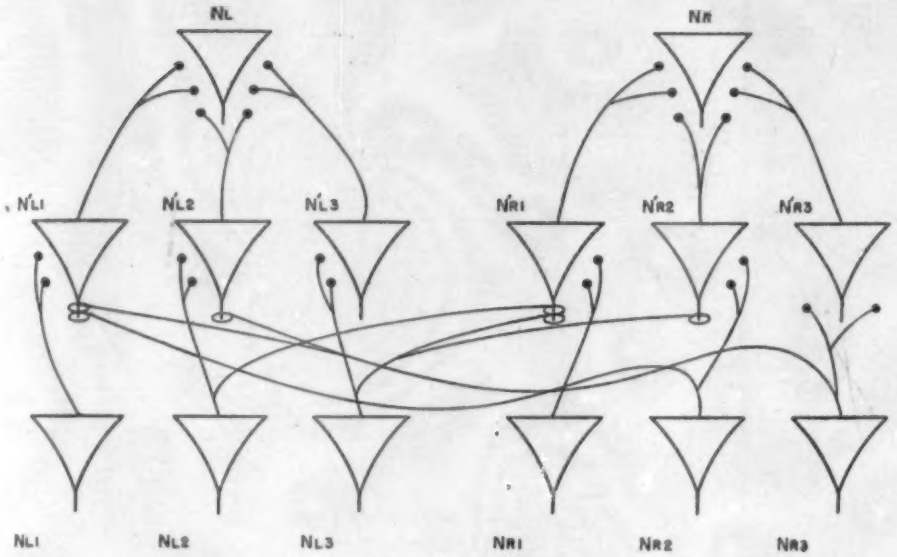

FIG. 4. Transposition

and higher; those of N_{L3} and of N_{R3} are equal and still higher. The N_{R1} are activated by the stimulus object lying to the right. Suppose the weak stimulus provided by C is capable of activating only N_{L1} among the sensory elements N_{L1}; the somewhat stronger stimulus provided by A can activate any of the N_{L1}. Similarly the stimuli C, B and A can activate N_{R1} alone, N_{R1} and N_{R2}, and all three N_{R1}, respectively. Then if B is presented on the left N_{L1} and N_{L2} both fire; if C is presented right, so does N_{R1}; but the inhibition from N_{L2} inactivates N_{R1}', whereas N_{L1}' and N_{L2}' will both fire, thereby activating N_L and producing a jump to the left. On the other hand, if A instead of C is presented on the right, N_{R2} and N_{R3} will fire and inhibit N_{L1}' and N_{L2}' (N_{L3} receives no impulses), while activating N_{R2}' and N_{R3}' to produce, ultimately, a jump to the right.

Thus transposition is perfectly explicable in structural terms. Doubtless the cycles involved in the learning itself must be activated from the efferent side of N_L and N_R, but their placement is made neither more nor less of a problem by the occurrence of transposition.

8. *Apparent displacement.* We conclude with one final structure for the apparent displacement of a perceived object, suggesting an incipient form of apparent motion. More complicated mechanisms could be given, but this will perhaps suffice for illustration. The experiment as described by Köhler (3) is this. If two lights are flashed, one slightly after the other, and not too widely separated, the second appears closer to the first when the flashes are in quick succession than it does when there is more time between flashes. In Fig. 5 the sensory elements N_1, N_2, N_3 and N_4 represent in their placement four retinal elements connected to them. The perceptual elements N_5, N_6, N_7 and N_8 represent by their placement the perceptual localization. If the firing of N_1 is immediately followed by the firing of N_4 and the silence of N_1, then the firing of N_4 will be followed by the firing of N_7 and the silence of N_8, owing to the inhibition of N_8 by N_A, and the concomitant impulses on N_7 from both N_A

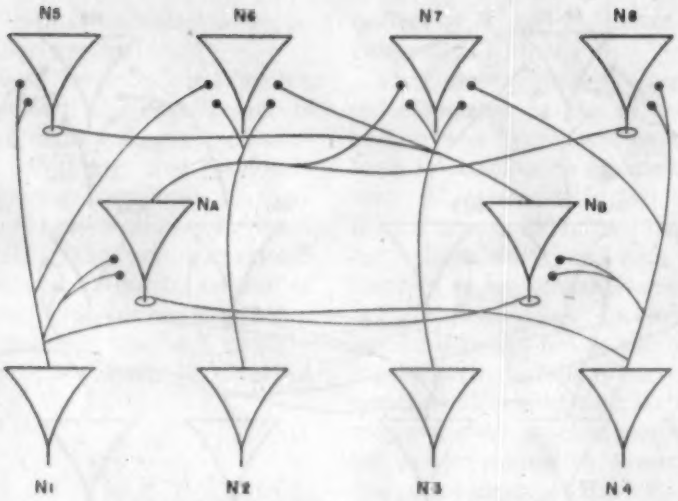

FIG. 5. Displacement with consecutive stimulation

and N_4. Note that with this structure, the subsequent activity of N_4 with N_1 inactive will result in a transfer of the activity from N_7 to N_8, whereas if N_1 and N_4 act together for a time, N_6 fires instead of N_5 and N_7 instead of N_8. The structure can be complicated so that N_6 and N_7 will fire only when the appropriate sequences occur (or when N_2 and N_3 are acting) and not when N_1 and N_4 fire together.

9. *Conclusion.* Whether the psychological processes considered are in fact mediated by such structures as these is, of course, problematic. Certainly our descriptions are greatly oversimplified. But the structures described seem to provide the basic requirements for the processes in question, and it seems clear that by adding elements, many of the attendant complexities can be accounted for even on the basis of our artificially oversimplified hypotheses.

It is perfectly possible that structural considerations will have to be supplemented in various ways for an adequate understanding of the more complex psychological processes. But it seems that an exploration of the potentialities of the structural theory is in order, not for the purpose of demonstrating its inadequacy, but for the purpose of ascertaining just how far it can take us.

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